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ORIGINAL ARTICLES.

THE MECHANISM OF INDIRECT FRACTURES OF THE SKULL.[1]

BY CHARLES W. DULLES, M.D.,

SURGEON TO THE OUTPATIENT DEPARTMENT OF THE HOSPITAL OF THE UNIVERSITY OF PENNSYLVANIA AND OF THE PRESBYTERIAN HOSPITAL IN PHILADELPHIA.

Hippocrates gives the earliest definition of the fractures we are about to study: The bone is broken in another part of the head than that in which the man received the injury, and the bone was stripped of the flesh. This definition remained unimproved until more than two thousand years later, when, in 1873, Félizet suggested the division of fractures of the cranium into "immediate and mediate;" if, indeed, this suggestion be an improvement. Hippocrates despaired of the cure of fractures remote from the seat of the wound, and seems to have had no more definite idea about them than that they might occur. The next author, Celsus, says: It sometimes happens, also, that the blow has fallen upon one point, and the bone is cleft in another. He furthermore recommends cutting down upon any point where there is softening and swelling, in case no fissure is found at the point struck, and bad symptoms arise. Here is a decided advance upon the teaching of Hippocrates, another being the observation of Celsus that the vessels of the brain might be ruptured without fracture of the cranium, anticipating a much later explanation of the term *"contrecoup."*

The Royal Academy of Surgery of Paris proposed in 1760, and again in 1765, as the subject for a prize, the following proposition: "To establish the theory of counterstroke in lesions of the head; and the practical conclusions which may be drawn from it." On the first of these occasions the prize was not awarded; on the second, it was awarded to Grima, who sent in for the second time the same essay which he had sent in for the competition, and which had been thought the best at that time. The same question was proposed for the prize of 1768, when prizes were awarded to Saucerotte and Sabouraut, for which Chopart also presented an essay, which was accepted and published by the Academy. Chopart not only recognized the elasticity of the skull, but exaggerated it, and, as a diagram in his essay shows, believed that a blow produced alternating and reciprocal shortening and lengthening of the axes, in a manner which some authors describe as undulatory. Nor would the history of this epoch be complete without mention of a valuable memoir upon counterstroke in other parts of the head, written by David, though published under the name of his pupil, Bazille—to which a double prize was awarded in 1771.

In 1844, Aran published an exceedingly valuable paper on fractures of the base of the skull, in which he formulated a theory which has been known by his name ever since. This theory was, that fractures of the base are always connected with fractures of the vault, from which they radiate, following a line within the zone in which the former occurred, and taking a course which corresponds with shortest route to the base. His conclusions are formulated as follows:

1. He never saw a fracture of the base, caused by a blow, without a direct fracture.

2. Fractures of the vault joined the base by "irradiation," and crossed any sutures in their way.

3. They followed the shortest route to the base (the curve of shortest radius).

4. They were ordinarily limited to certain regions (corresponding to the several great fossæ of the cranium), and followed a certain direction.

5. Fractures of the vault sometimes coincide with *independent* fractures of the base; but only when there is considerable comminution (*ébranlement*) and very multiplied fractures.

In 1847, Guthrie published a monograph on injuries of the head, in which he gives an excellent *résumé* of the history of various sorts of fractures of the cranium. Among them he speaks of counterstroke, the occurrence of which from temporal to temporal, or from parietal to parietal, he doubts, but says that fracture of the base from blows on the vortex or occiput are among the commonest accidents of surgery.

In 1853 was published, in *Guy's Hospital Reports*, a first instalment of Mr. Hilton's sagacious lectures on the cranium, which was followed, in 1855, by their publication entire in a separate volume, by Dr. Pavy, after their revision by Mr. Hilton himself. In these lectures Mr. Hilton explains fractures of the skull upon the "vibration-theory," and speaks of the posterior clinoid processes and the extremities of the petrous bone as points to which vibrations are conducted, and where they are transmitted to the cerebro-spinal fluid, thus being "interrupted or lost before reaching the cerebral tissues," etc. The fallacy of this conclusion I shall have to consider hereafter.

[1] Abstract of a paper read before the College of Physicians of Philadelphia, on February 3, 1886.

In 1854 the keynote to a new theory of indirect fractures of the skull—which had, however, as we have seen, been foreshadowed much earlier by certain observers—was struck in Germany by experiments made by Bruns, of Tübingen, to determine the elasticity of the cranium.

In 1864, Chauvel published a thesis on fractures of the cranium, founded upon a careful study of its anatomy, in which he laid great stress upon the elasticity of the skull, and described the "vibrations" produced by a blow, as the repeated and alternating shortening of the prime axis and lengthening of the transverse axis. He called attention to the fact that the petrous bone, although the hardest in the cranium, is the one most frequently fractured. He described three kinds of fracture: 1, direct; 2, contra direct; 3, indirect. The first kind is simple enough. The second occurs opposite to the point struck, and he offered no explanation of them, except the comparison of those caused by the spinal column to the manner in which the handle is driven into the head of a hammer by striking the remote end of the handle against some resisting body. The third form of fracture he defined as that occurring between the point struck and that directly opposite. This he found in eleven out of sixty fractures.

In 1872, Schwartz analysed 115 cases, in most of which the history was very incomplete, and he found fracture of the base alone, without fissure of the convexity, only seven times. He found about thirty-nine per cent. of the fractures limited to the zones described by Aran and Hewett; while in over sixty per cent. this regularity was wanting. This is one of the most important features of his investigations. Another is that the study of fractures he had analyzed showed that the petrous bone was fractured in 66 out of 115 cases; and that force applied to the side of the head caused fractures somewhat parallel to the axis of the bone, while force applied to the occiput, or forehead, or vertex, caused fractures transverse to the axis. This was an important contribution to the accumulating material for a new consideration of the mechanism of indirect fractures of the skull, as we shall see hereafter.

In 1873, Félizet published an elaborate illustrated monograph[1] on the subject of fractures of the cranium. He regards this as a solid case, of a spheroidal shape, but claims that its internal configuration renders inaccurate a comparison of it to any geometrical figure whatever. By experiment he demonstrated that the dura mater makes the skull where it is attached more resistant to fractures caused by flattening its curve. Experimenting with a billiard ball let fall from a height, he found that its equator was increased, that this increase was greater in a direction at right angles to its fibres than in a direction parallel to them. The result was an ellipsoidal equator, and when a fracture occurred, this crossed the long diameter at right angles. He unwittingly paraphrases the theory to which I have several times alluded, and which I shall soon consider, in saying, "We know that a fracture results from depression of the curved surface situated between two resisting pieces, and we know, also, that every effort tending to depress this curve divides into two forces, the one a driving force (*force de tassement*), which acts on the axis, or near to it, of the supporting walls; the other, a disruptive force (*force de glissement*), which acts transversely to them, and tends to separate the two extremities of these pieces." He compares the separation of sutures, the result of blows, to that which is effected in anatomical preparations by filling the skull with dry peas and then soaking them, so that by swelling they tear the skull apart. He declares that there is a perfect analogy between this and what he calls the "*grand fracas*" (grand smash), in which he says "the bones, compressed over a large surface, rock (*basculent*), and this rocking movement sets up violently a dragging force perpendicular to the plane of the surface of union of the bones of the cranium." As we shall see hereafter, this is the same as saying that a force which depresses an elastic curved figure, will enlarge its diameter, and be converted into a disruptive force acting at right angles (perpendicular) to the meridian. Herein Félizet comes near to the theory which the Germans call the "Berstungs" (bursting) theory, although he does not quite reach it, and may perhaps, even now, not see his statements to be so strong a confirmation of it as they appear to me to be.

The term "*contrecoup*" Félizet rejects, and would adopt that of M. Beau, of Brest, who called indirect fractures "mediate." Félizet compares the mechanism of these fractures to those produced by a blow upon a wedge or a chisel. The part broken, he holds, is directly struck by another part, which in its turn, received a blow and transmitted it. Yet he admits that, very rarely, these fractures by *contrecoup* do occur. He divides fractures of the cranium into direct, indirect, and mixed. Indirect fractures include those which are independent. In respect to the application of the violence which causes them, fractures may be divided into immediate and mediate. His conclusions may be briefly stated as follows: all fractures not immediately limited to the point of impact follow certain definite routes, to which they are restricted by the added strength of certain parts of the skull, which depends upon the presence of certain "*murs boutants*" (buttresses): the petrous bones, the orbito-sphenoid prominences, the occipital protuberance, and the naso-frontal protuberance. The basilar process and the antero-lateral parts of the occipital bone remain intact amid all fissures and constitute a "centre of resistance." The vibration theory, he says, is disproved by experiments, the fundamental phenomenon is a violent flattening of part of the vault, and a separation of the resisting portions (*pièces de résistance*) which support it. When the violence is perpendicular to the surface of the cranium, fractures radiate directly to the base. In this, it will be seen, Félizet agrees with Aran. Finally, he admits that these are fractures, called by "*contrecoup*," the mechanism of which escapes us completely.

The opinions of Félizet are founded upon a careful analysis of a number of pathological specimens and upon a number of experiments upon fresh skulls.

[1] Félizet, G. Recherches anatomiques et expérimentales sur les Fractures du Crâne. 8vo. Pp. 167. Paris, 1873.

The value of his work can be appreciated only after a thorough study of it. The result of it was to bring him to a conclusion which may be succinctly stated as that indirect fractures are due to a disruption by splitting. There can be no doubt that the course of a fracture will be determined to a considerable extent by the reinforcements which the skull has, in certain places, from its buttresses (*murs boutants*); but, as we shall see hereafter, these very buttresses are often broken both transversely and longitudinally, and Félizet's centre of resistance is by no means a centre of immunity, but an exceeding frequent seat of fracture.

A few years later Baum[1] opposed the vibration theory, and made some experiments by strewing sand on a skull and applying a vibrating tuning-fork to it. From these experiments he concluded that vibrations were restricted by surrounding less elastic parts, like the vibrations of a drum-head. The petrous bone, however, he concluded, owing to its anatomical structure and relations, vibrates like a rod held by one end in a vise. He refers to Weber's wave theory, and concludes that blows upon the skull do not furnish the conditions of repeated impulses which produce a high vibratory action. By compression of a few skulls in a suitable frame he got results which correspond to those of later German experimenters, who adopted the "bursting" theory.

In 1878, P. Bruns attributed indirect fractures to the elasticity of the skull, as described by von Bruns in 1854, and explained the frequent occurrence of these fractures in the base of the skull by the fact that this is the weakest and most fragile part of it. In the same year, Perrin made some very instructive experiments, in which he attempted to imitate the usual conditions of the skull by protecting it with an elastic cushion of cotton wadding or of caoutchouc and then throwing it upon a stone pavement, or by putting his cushion on the pavement itself. He always got direct fractures by blows, and by precipitating the skull he usually got indirect fractures. He showed typical specimens to the Société de Chirurgie, in all of which the fractures were indirect and meridional, and in some interesting cases the vitreous alone was involved in certain places. He found that blows on the vertex or occiput usually cause fractures by contrecoup (which he defines as fractures occurring at a place other than that struck), direct fractures being rare exceptions; while blows on the forehead, the parietals, or the temporal bones, rarely cause any but direct fractures.

In 1880, Bergmann published another valuable contribution to this subject. He compared the elastic conditions of the skull to those of the thorax, which, when compressed or struck in one part, breaks in another. He also compares the mechanism of fractures of the skull to the bursting of the eyeball under pressure, referring to Arlt's demonstration, that when compressed the eyeball bursts in the equator. Fractures from severe blows, he believes follow this law; those dependent upon moderate force follow the law laid down by Félizet. He calls attention to the importance of duly estimating the variations in amount, direction and velocity of the force applied to the skull, and the support afforded by strengthening bones of the skull and face. He cites Baum (whom I have cited) as refuting the vibration theory, and seems to have the idea that indirect fractures of the skull are to be attributed to a disruptive force, and not to a propagation of vibrations.

In 1880, Messerer conducted a series of experiments which demonstrated more accurately than had ever been done before the elasticity of the cranium, and which made clear the fact that when the skull is compressed in one axis, the circumference at right angles to it is enlarged.

In 1881 Nicolai Hermann published an inaugural dissertation on fractures of the base of the skull, in which, besides an analysis of seventy-five cases gathered from various sources, he gives an account of seventeen experiments in which he compressed the skull in longitudinal, transverse, and diagonal directions, until it broke. His experiments always resulted in a fracture parallel to the direction of the compressing force. He believes that fracture always begins at one of the points compressed and travels away from it. By analyzing the cases he had collected he concluded that they supported his deductions from experiments. It seems to me that some of his figures show plainly that the fissures must have begun at the middle and travelled both ways. In the same year Julius Schranz published an account of thirty-four experiments upon skulls, some (thirteen) from which the vault had been removed, but the most of them entire, in which he endeavored to imitate the conditions in which fractures are caused by different sorts of violence. The experiments were interesting and instructive. The great majority of the fractures he produced were splitting fractures, and *not* immediately connected with the point to which the violence was applied. In a considerable number isolated pieces of the vitreous table were split off, or the dorsum ephippii. These (and other indirect fractures), he says, can only be explained by the vibration theory. A curious result of his experiments was that the brain was injured only once, and the dura mater separated from the bone only three times!

But it remained for Von Wahl, in 1883, to utilize the material which had been recently gathering, and to formulate unequivocally the theory that fractures of the skull may be divided into: 1, crushing fractures, in which the line of fracture runs at right angles to the axis of the force applied; and 2, bursting fractures, in which the lines of fractures are parallel to the axis of the force which gives rise to them, and which begin, in the words of Messerer, at some point in a line which, like the equator, in relation to the poles of the earth, circumscribes the hollow figure in a plane equally distant from both points of compression. He claims that, in compression of the skull—which may be gradual, as in a vise, or sudden, as when effected by a fall or blow—its elasticity, *in toto*, comes into play, and while its diameter which is parallel to the force applied is short-

[1] Baum, W. Beiträge zur Lehre von den indirecten Schädelfracturen. Langenbeck's Archiv f. klin. chir., Bd. xix, pp. 381–399, 1876.

ened, its diameters which lie at right angles to this are lengthened. The result is, that indirect fractures run in lines which we may describe as meridional. His conclusions rest not only on theoretical considerations and a review of the testimony and opinions of others, but also upon his own clinical experience, and a series of ingenious experiments, which are fully described and beautifully illustrated in his monograph, a careful study of which may, without injustice, be said to be indispensable to one who would form a direct opinion upon the subject of which it treats.

In 1884, Dr. Nancrede, of this city, published an elaborate and valuable paper on "Injuries of the Head,"[1] in which he adopts the vibration theory, and supports his opinions with some interesting anatomical and physical considerations. He regards the base as a stronger part of the cranium than the vault, speaks of the brain as lying on a water-bed, and describes vibrations of the bones as travelling by the nearest "anatomical" route to the base, there to be discharged, like electricity, at certain points—the ends of the petrous bones and the clinoid processes—into tissues which are non-conductors of vibration. He also calls attention to the important influence which the position of the head has upon the direction in which a force applied to the condyles will be conducted. In a very interesting case, which he cites from his own experience, there was a separation of the masto-occipital suture, which he says was forced apart "as if from within."

In 1884, Messerer published a second and most admirable paper on fractures of the cranium in which he gives a description and analysis of eighty-two experiments on fresh skulls, sometimes on entire cadavers. His experiments included accurately regulated blows upon skulls, detached or resting on the spinal column, and falls of the skull upon a hard base. These experiments completely refute the laws of Aran as well as those of Félizet, and seem to show conclusively that indirect fractures of the skull are dependent upon separation of the meridians caused by the depression produced at the point of contact by a blow.

Finally, our sketch of the history of this subject is brought down to the present year by a reference to the paper of Greder, published in the early part of 1885.[2] The experiments of Greder were made upon skulls connected with the trunk and still covered with their integuments and containing the brain, by blows with a weight of 6400–7650 grammes (about 12–15 pounds). After a large number of these experiments, some of which were most ingeniously varied, Greder comes to the conclusion that solutions of continuity of the base of the skull are to be regarded as the result of bursting; that the direction of fissures is parallel to that of the force causing them; that the extension of a fissure is dependent upon so many intercurrent conditions peculiar to each skull, that it can at most be only suspected from the intensity of violence. As to the point at which the burst begins, he holds that it is not always in the equatorial line, nor always at the point of impact, but at that point where resistance to the disruptive force is least. This will happen, he thinks, in the majority of cases, in the equator or in its neighborhood.

[1] International Enclopædia of Surgery. Vol. v. pp. 1–109.

[2] Greder, Wilhelm. Experimentelle Untersuchungen über Schädelbasisbrüche. Deutsche Zeitschrift für Chirurgie, Bd. xxi., 5 und 6 Hefte, 9 März, 1885, pp. 491–510, tafeln vii.–xiv.

THE ELASTIC PROPERTIES OF THE SKULL.

In 1854, von Bruns demonstrated the elastic properties of the skull by a series of experiments which only required multiplication and variation to confirm what an important part the elasticity of the skull plays in the production of indirect fractures. Since then, almost innumerable experiments and studies of specimens of fractures of the skull have coöperated to establish the conviction that, with certain exceptions, the mechanism of indirect fractures of the skull may reasonably be explained according to what the Germans call the "bursting theory," *i. e.*, to the conversion of a direct depressing force into an indirect disruptive force, produced by a shortening of the axis parallel to the direction of the force and a complementary lengthening of the axis at right angles to the former.

The experiments of Messerer can be analyzed so as to throw a great deal of light upon the physical properties of the skull, revealing the parts in which it is strongest, and those in which it is weakest. But, in order to understand their significance, we must have a clear idea of the mechanism by which, in a hollow case, the shape of which has a general resemblance to that of a spheroid or ellipsoid, a compressing force is converted into a disruptive force. It will be readily seen that when an elastic spheroid is compressed in any diameter, all the diameters lying at right angles to this, that is to say, in planes parallel to that of the equator, must be elongated. The experiment of pressing on the convex side of a bow, the ends of which rest on the ground, will illustrate what takes place in every chord of every arc that is depressed. It is equally clear that the elongation of the diameters lying in any plane will elongate the circumference of this plane, because a circumference of longer radius is longer than a circumference of shorter radius. As a result of this elongation of the circumferences parallel to the equator in any spheroid or ellipsoid, the meridians passing from pole to pole will be separated in a direct ratio to the extension of the different diameters and circumferences. This operation is illustrated whenever an umbrella is raised.

As a consequence of the separation of the meridians in a hollow sphere or ellipse, particles which before occupied a certain space between two meridians, will be compelled to separate in order to occupy the increased space between the meridians. The consequence will be that, in every case such as we have supposed, there will be a struggle between the cohesion of the particles and the disruptive force due to the separation of the meridians, and whenever the disruptive force overpowers the cohesion of the particles lying along and between two of the meridians, a solution of continuity will take place. The disruptive force will be at a maximum at the equator, but the same conditions of disruptive force opposed to cohesion will be found in different proportions in every line parallel to the equator. As a result, in a perfect sphere with homogeneous walls such a solu-

tion of continuity would naturally begin at the geometrical equator and extend equally and simultaneously in opposite directions in a meridional line toward the poles. But in a spheroid or ellipsoid not regular in shape, and with walls varying in strength in different parts, the result would be modified by these variations. This is exactly what we find in the case of the skull, which, while bearing a certain resemblance to an ellipsoid, does so in only a modified way, and can only be expected to exemplify the law just stated subject to modification due to its own peculiarities.

With this in mind, let us see what the experiments of Messerer show. Messerer found that the skull burst in the base under an average pressure 650 kilogrammes applied in a longitudinal direction, and under an average pressure of 520 kilogrammes applied in a transverse direction. From this we may draw two very important deductions. First, because the burst took place in the base.

Corollary I. The cohesive power of the base of the skull is less than that of the vault.

Second, because the skull burst under a pressure of 529 kilogrammes applied transversely, and under a pressure of 650 kilogrammes applied longitudinally.

Corollary II. Cohesion is less in the coronal zone of the skull than in the sagittal zone—that is to say, the skull is less able to resist a disruptive force due to blows upon the vertex or sides in the coronal zone than it is to resist a similar force caused by blows applied to the forehead or occiput. This is not only a fair inference from the experiments of Messerer, but it also accords with clinical observation.

Messerer found again that compression of the skull in a transverse direction, with a force of 520 kilogrammes, diminished the transverse diameter 4.4 millimetres; and that compression in a longitudinal direction, with a force of 650 kilogrammes, diminished the longitudinal diameter only 2.7 millimetres. From this we may deduce

Corollary III. The skull is more compressible in the transverse diameter than in the longitudinal diameter. (This fact tends to strengthen the deduction of Corollary II.)

Messerer found, also, that compression of the skull in the transverse diameter, with a force of 520 kilogrammes, increased the longitudinal diameter 0.4 millimetre, and the perpendicular diameter 0.6 millimetre. From this we may deduce

Corollary IV. The skull is more extensible in the perpendicular diameter than in the longitudinal diameter of the sagittal zone. The natural inference from this corollary is that, under the influence of a force applied to the sides of the head a fracture which would naturally begin in some point in the sagittal zone, will begin in that part of it which is intercepted by the perpendicular diameter more readily than in that part of it which is intercepted by the longitudinal diameter—that is to say, at the base or vertex rather than at the forehead or occiput. We have already seen (Corollary I) that the cohesion of the skull is less at the base than in the vault, therefore, of the two points just indicated, the base is that of election. Here, again, clinical observation supports our deduction.

Messerer found, further, that compression of the skull in the longitudinal diameter with a force of 650 kilogrammes increased the transverse diameter 0.6 millimetre, and the perpendicular diameter 0.1 millimetre. From this we deduce

Corollary V. The skull is more extensible in the transverse diameter than in the perpendicular diameter of the coronal zone. (This is the converse of Corollary III.) The natural inference from this corollary is that, under the influence of force applied to the forehead or occiput, a fracture which would naturally begin at some point in the coronal zone will begin in that part of it which is intercepted by the transverse diameter more readily than in that part of it which is intercepted by a perpendicular diameter—that is, at the sides rather than at the base or vertex. We have already seen (Corollary I) that cohesion is less in the base than in the vault; therefore, a fracture which, for the reasons just stated, would naturally begin at the base of the skull would be more likely to occur near the base than near the vertex. In estimating this probability it may also be borne in mind that the horizontal equator of the skull lies nearer to the base than to the vertex, the curvature of the former being much less than that of the latter. So that the point of election for the fractures we are now discussing would naturally lie low down on the side of the skull. This deduction also is supported by clinical observation.

Messerer found, again, that compression of the skull in a transverse diameter, with a force of 520 kilogrammes, increased the longitudinal diameter 0.4 millimetre and the perpendicular diameter 0.6 millimetre; and that compression of the skull in the longitudinal diameter with a force of 650 kilogrammes, increased the transverse diameter 0.6 millimetre and the perpendicular diameter 0.1 millimetre. From this we may deduce

Corollary VI. The skull is more extensible along the sagittal equator than it is along the coronal equator. We have already seen (Corollary III) that the skull is more compressible in the transverse diameter than it is in the longitudinal diameter. It is, therefore, seen that the equator along which the skull is more extensible (the sagittal) is that one the plane of which is cut at right angles by the diameter in which it is more compressible (the transverse), and *vice versa;* which furnishes in the skull a demonstration not only of the elongation of the equator which is produced by depression of the poles of a spheroid, but also of the direct ratio of this elongation to the depression. Furthermore, it appears from the observation of Messerer just cited, that a smaller force applied to the skull in a transverse direction will produce a greater elongation of the corresponding equator than will be produced by the application of a decidedly greated force in a longitudinal direction. From this we may draw the deduction that force applied to the side of the head is more likely to produce a fracture crossing the sagittal equator than force applied to the forehead or occiput is to cause a fracture crossing the coronal equator. This deduction is in accord with the results of Messerer's experiments, in which the skull burst under a press-

ure of 520 kilogrammes applied in a transverse direction, and only under a force of 650 kilogrammes applied in a longitudinal direction. It is also in accord with clinical observation.

We have now analyzed Messerer's experiments so as to ascertain the points of election for disruptive fractures which may occur in (that is, across) two of the principal equators of the skull, the sagittal and coronal. It is not possible to discover in the same way the points of election in the horizontal equator of the skull; for Messerer found that compression of the skull in the vertical direction, between the vertex and the spinal column, led to a direct driving in of the base under so small a pressure as 270 kilogrammes.

The results of our analysis of these experiments may be summarized as follows: 1. Bursting fractures are more likely to occur at the base of the skull than in the vault. 2. Force applied to the side of the head may be expected to produce a bursting fracture crossing the sagittal equator. Such a fracture is more likely to occur in the base than at the vertex. 3. Force applied to the forehead or occiput may be expected to produce a bursting fracture crossing the coronal equator. Such a fracture is likely to occur in the temporo-parietal region, and nearer to the base than to the vertex. 4. Force applied to the vertex or base of the skull may be expected to produce a fracture at the base.[1]

(*To be concluded.*)

TUBERCULOSIS PULMONUM, ACUTE AND CHRONIC —ITS NATURE AND TREATMENT.

BY A. S. v. MANSFELDE, M.D.,

OF ASHLAND, NEB.

PERMANENT SECRETARY, NEBRASKA STATE MEDICAL SOCIETY; PRESIDENT OF THE RAILROAD SURGEONS' SOCIETY OF NEBRASKA, ETC.

(*Concluded from page 313.*)

We are now prepared to follow the development as it occurs in the lung, as well as appreciate the conditions which form its clinical picture—and perhaps draw conclusions for the treatment of the disease which approach somewhat nearer the goal striven for—its cure.

Grant that the soil for the development of the disease has been furnished in accordance with the requirements above indicated, it is still absolutely necessary that the seed be furnished for the harvest of death—and this is supplied by the bacillus of Koch, the bacillus tuberculosis. The writer is forced by the array of facts upon the one hand, and the absence of one authenticated case of inherited tuberculosis upon the other, to join the army of those who believe that the lesions, *i. e.*, the anatomical parts of our disease, may and do exist without the presence of the bacillus, but tuberculosis never occurs or exists without the bacillus of Koch.

Here it may be well to state (in order to prevent confusion) that the occurrence of tuberculosis in the embryo has been demonstrated, and the author is well aware of the fact; but he also knows that in such cases *the embryo invariably presents the primary lesions in the liver*, the organ with which the mother's blood first comes in contact, *and that the mother is always tuberculous herself;* and the writer has no doubt that in such mothers tubercle bacilli can be detected in the blood. But who has ever heard of a healthy mother, having cohabited with a tuberculous husband, giving birth to a tuberculous child? The bacillus tuberculosis, therefore, is the most important factor in the establishment of the disease; without it tuberculosis is non-existent, however many tubercles may have been formed by the atomization of cheese, flour and other things. We will not stop to dwell on the causes which make the apices of the lungs the favorite seat for tuberculosis; presumably mechanical forces play the greater, if not exclusive rôle in this, of which by far the most important seems the gravitation of the blood in the lung towards the middle and lower lobes. We have already observed that ischæmia is an important adjuvant in the development of this disease. The seed, the bacillus, and in the very nature of things, more often is spores, contained in the form of dust in the air inhaled, finds its way into the lungs, or, more properly speaking, is imbedded upon a spot denuded of its epithelium. (For it must be remembered that normal epithelium offers an insurmountable barrier to the ingress of bacilli, if not of their spores also.) Or, what is more frequently the case, the seed is sown upon the contents of the alveoli, or, more definitely stated, the acini of the lung, the food consisting of fibrin, alveolar epithelium and leucocytes, and they grow and multiply, when they are prepared to attack the surfaces denuded of their epithelium, and in this wise:

The bacilli find their way into *lymph spaces*, and preferably those connected with the base of the lobulette, or acinus, simply because they are the most abundant in this situation. The advent of the bacillus is the signal for the graphically described "battle of cells with bacteria." The leucocytes endeavor to eat the intruder (and this must not be considered a fiction, for it has been observed taking place), and if successfully accomplished, as no doubt often happens, the description would end with the death of the hero—but, unfortunately, the bacillus will not only triumph, but in conquering it grows and multiplies. Other leucocytes now endeavor to devour both the bacilli and their victim, and in like manner fall an easy prey to the parasites. Thus quickly a conglomerate of cells (a ball, a tubercle) is formed, the type of the recent tubercle, minus the giant cells. And the absence of the giant cell in these formations is not an accident, but an invariable rule. It is otherwise when the bacillus is attacked (or attacks?) by the aforementioned endothelial cells of the lymph spaces, or the amœboid cells of the fibrous tissue. Here a somewhat different manœuvre ensues. The bacillus finds its way into the cell, or is taken up by the cell, we do not know which; and now, by the emanation from its economy of a poison of the nature of ptomaines (D. L. Brieger), or by the supply of a ferment (capable of being furnished by all albuminoid

[1] It may be worth while to call attention to the fact that there is no essential difference between a blow applied directly upon the vault or side of the skull and one transmitted through the spinal column by a fall upon the feet or buttocks.

substances of cells), the part of the cell *on* which, or *in* which the bacillus is lodged necroses by a typical coagulation of its substance (Prof. C. Weigert). That this is true necrosis, namely: the partial coagulation, is evident from the fact that the nucleus not only remains alive, but its adherent protoplasm accumulates and furnishes the means for the segmentation of the nucleus; but whilst this occurs in the one cell, another, and more often many cells, not only pass through the same changes, but adhere, as would be expected, to the *necrosed* part of the first cell so affected, thus forming a plexus of a very peculiar form, *i. e.*, centrally a mass of necrosed tissue, surrounded with the living protoplasm of cells, which contains one or more nuclei, of course peripherally placed, like rosettes or wreaths, when the whole conglomerate is considered, this is the typical *giant cell* of Langhaus.

And just as in the tuberculous ulcer the bacilli occupy the ground between dead and living tissue, so here we find in the giant cell, the bacilli between the necrosed central portion and the periphally placed nuclei, which, as is known, always disappear when the cell has become the victim of the coagulation necrosis. In fact, even the bacilli are lost to view in this dead part of the growth, having, as some writers assume, suffered destruction, only with this difference, that they have left their progeny, the spores, behind (which has not yet been demonstrated by staining media), to serve in their turn the purposes of a renewed invasion of the adjacent tissue. Exterior to this conglomerate of cells, whether composed of leucocytes only, or of giant cells and leucocytes, the consequences of an irritation of the tissue always appear in the form of an invasion of multiplying tissue cells and wandered-in white blood-corpuscles; to give rise to the attempt at organization most always noted upon the periphery of slowly forming tubercles and masses of tubercles. And this attempt at organization succeeds in the ratio of the blood supply to the parts, and their own normal constitution, and fails as the parts approach the composition described by Formad, and known as the scrofulous. If it succeeds, then the wall of cicatricial tissue built around the tubercle prevents the invasion of the surrounding tissues by its contents, the spores of bacilli particularly, after they have been set free by the softening of the tubercle. And this holds good not only for the single tubercle, but also for any number of them, and, indeed, whether they exist upon a free surface, or upon the wall of a cavity which is of their creation.

As already stated, the tubercle grows peripherally, *i. e.*, the central one having softened, its contents rapidly invade adjacent parts to add like growths to the central mass. And so minute are the single growths, that a compound tubercle of the size of a poppy seed is made up of at least *thirty-six* submiliary nodules (Rindfleisch). Another method of peripheral enlargement consists in the coalescence of adjoining tubercular masses—this concentric and peripheral growth proceeds until the base of a terminal bronchus is surrounded by a tubercular mass which is known as Carswell's grapes. Hitherto, as is often the case, tubercles have been, and most unjustly so, described and figured as the cause of the disease, and yet they are only so many monuments to friendship, unwise friends, we are willing to admit, who have in their component parts, the cells, paid their friendship with their death.

Tubercles are not disease, but nature's method of staying disease and death, for as long as they themselves remain untouched by decay they are harmless to the economy. Yet, like too assiduous friends, they may overdo their good offices (though we suspect it is the fault of the anatomical relations of the parts and the importance of their functions to the economy) —they grow until not only the bronchial twig but the pulmonary vessels, the artery in particular, are strangulated either by the masses which surround them, or by the invasion of the tubercles into them, and in both instances, though not a typical embolus results, the consequences are exactly the same, the tissue dies invariably, and when removed it leaves a cavity. This is the case whether the process proceeds from the surface of a mucous membrane, as that of the intestines, or from the situation just described. We said a *cavity* purposely, and not an *ulcer*, for this, after the necrosed tissue is removed, presents a granulating surface bathed in pus, indicative of an effort at redemption. Whilst the wall of the cavity presents nothing but necrosed and dying tissue, made uneven by larger and smaller masses of tubercles in all stages of development and decay. This is the picture of chronic tuberculosis pulmonum.

Acute miliary tuberculosis of the lung presents in its causation an entirely different aspect, though the final issue is identical with the one just described. *It consists most generally in a multiple embolism of the capillaries of the pulmonary artery, induced not by bacilli or their germs, but by debris from softened foci (glands more often) with bacilli in and on them, which have found their way into the circulation.* In this causation two other sources must be included: direct injection into the circulation of the virus in sufficient quantity; and reversion of the lymph stream. Inoculation of the virus belongs in the first category, as miliary tuberculosis is noticed only after the formation of tubercles *in loco*. Direct injection self-evidently reverts to the first category also, the place of the introduction of the poison not being considered. Reversion of the lymph stream as a cause of miliary tuberculosis of the lung has only of late obtained a safe foundation by the observations of v. Recklinghausen. In cancer of the bronchial glands, it is not seldom that metastases are found in the subpleural tissue. The same transmission must be admitted for broken-down tuberculous material from the same glands. The infective embolus, for such it is, provides all the conditions we have found in the bronchi, and have claimed as necessary for the multiplication of the bacilli and their succeeding destructive work. All subsequent infiltration of the tissues is identical with that of the bronchial disease, with this difference only, that in the latter the infiltration is primarily located in tne *peribronchial* lymphatics, whilst in the former it is found in the *perivascular* lymphatics, and that in this the processes are generalized, and in the other they are lo-

calized. And finally, that infection of both lungs ensues from all the sources mentioned but one, reversion of the lymph stream, in which the general tubercular formation is unilateral in the earlier stages of the disease.

To strengthen the scientific basis of the treatment of tuberculosis we must mention the freedom from this disease enjoyed by persons with lesions of the heart, of which mitral stenosis is the one most rarely complicated with pulmonary tuberculosis. Ruehle, in Ziemssen's "Cyclopædia," page 507, says: "The correctness of the general statement must probably be admitted." And after giving Traube's view, that the more copious effusion from the pulmonary vessels, whereby the dessication and caseation of morbid products are promoted, is perhaps the reason for this exemption from the disease, he dismisses it "because a discussion of *hypotheses* (italics mine) is out of place here." I certainly agree with him as to the impropriety of the discussion of hypotheses in a lengthy paper like his or ours. Yet the great clinician's (Traube's) view can no longer be classed with hypotheses. It is a fact that in all the lesions of the heart which seem to lessen the occurrence of tuberculosis pulmonum the right ventricle is hypertrophied, and this hypertrophy is not caused by an interference with the blood current between it and the lung, but between the lung and the general circulation, within the province of the left heart. The changes produced, in consequence of these lesions of the heart, in the lung, are strictly in keeping with the conditions required to prevent the occurrence or the development, to any extent, of the lesions of tuberculosis in the lung. It is a clinical fact that the hypertrophied right ventricle will induce congestions in the lung, particularly if the blood stream is impeded on the left side of the heart. This chronic pulmonary congestion is followed by two sequelæ—an interstitial connective tissue hyperplasia (cirrhosis of the lung) and an increase of the velocity of the lymph stream, both conditions which our exposition will warrant us in accepting as diametrically opposed to the development of tuberculous lesions. Indeed, the latter function, the ready flow of lymph, is so very important, that its maintenance and its increase become the momentous questions in a rational treatment of the disease. That this is normally brought about by congestions, by the active contractions of the muscles of a part, by deep and often repeated in and expirations, and by the normal relation of the histological elements of the tissue, needs no demonstration.

Stress is here put upon the artificial increase of this function by Cohnheim's experiment. He observed the lymph stream from the thoracic duct increased twenty-five times, when he injected into the circulation forty per cent. of the body weight of a $\frac{6}{10}$ of one per cent. solution of chloride of sodium in distilled water. Much, indeed, has been said of the prophylactic treatment of tuberculosis, and much more is yet unspoken, and far more unheeded. Prevention in this disease simply means a shutting out from the economy of Koch's bacillus. To enumerate the means by which this ought and can be done, would necessitate the incorporation in this paper of everything that hygiene has suggested to keep air pure as the breath of life. The worst of all breeding-places for tuberculosis is the crowded work-shop, illy ventilated and overheated, and in which work is performed which produce dust full of irritating particles. The least offensive place is a mountain home, in an equable, dry and rather cool climate, where you are your own neighbor, and the only one. To enumerate the methods of infection would prove an equally herculean undertaking. From the mother's blood (Johne) and her mouth and lips (Herterich), from inoculations through trivial injuries, as a cut in the finger (E. A. Tscherning), or the dentist's forceps (the author); from inhalation of the poison in home, school, work-shop, crowded hall and prison cell, from food and clothing, tuberculosis may take origin. It is certainly very careless, nay, criminal, if the author's explanation why measles and pertussis are so likely to be followed by tuberculosis (and this is a fact) is correct, to permit a child sick with these disorders to remain in the company of a person suffering from tuberculosis. It should never be allowed if it can be avoided.

The cure of diseases and conditions which favor the development of tuberculosis forms, of course, a very important part of the prophylaxis of the disease. Besides this, any and all means which favor the restoration or the maintenance of the highest vitality to the elemental parts of the lungs, as well as of the whole body, are of the greatest importance not only in the prevention, but also the cure of tuberculosis. It is self-evident, from what has been said, that the specific cure of tuberculosis demands, in the first place, the destruction of the bacillus. Much has been suggested and done in this direction to warrant the hope of final success. This presumably lies in the direction of inhalation of parasiticides, and their injection by the nearest and most direct channels practicable. And if these inhalations and injections contain the means (alkaline or neutral solutions) for the solution of the tubercles, then the cure of the disease will become a scientific certainty. It is not the object of the writer to burden medical literature with an exhaustive essay upon tuberculosis; such have been furnished often and by abler writers. He will therefore not be charged with omissions—that mention has been made in a general way only, of the treatment which to-day receives universal sanction.

The heart should be induced to do its best by stimulants and tonics, to establish the conditions described at length, which will curtail the disease, namely: remove impediments to the free circulation of fluids in the lung and favor cicatricial growths around tubercles and their cavities.

The sewers of the body, particularly the kidneys, should not be neglected, rather stimulated to remove all effete matter promptly and thoroughly from the body. Enlarged glands, wherever situated, particularly on the neck, and in reach of the hypodermic syringe, should be made harmless by injections of iodine and carbolic acid (as has been twice successfully done by the writer). Cod-liver oil should never be omitted, because of its undoubted influence in removing or preventing the formation of scrofulous tissue.

In what has been said no attempt has been made to explain the manner or cause of the formation of "scrofulous tissue." That it is acquired *intra vitam* no reasonable doubt can be entertained, and for its inheritance an array of facts speak, which are invincible. The *why* in both instances is yet *sub judice*. The writer has little doubt that in its proper time it will be shown that the digestion, assimilation and reappropriation (burning) of fat in the economy, and particularly its relation to the proper maintenance and rejuvenation of the tissue elements furnish the field upon which the pathologist will have to seek for a solution of this anomaly of the connective tissue and its lymph spaces. Even the crude conception of oil as a *lubricant* for man's economy (which is so often likened to an engine), opens a vista of thought far too vast for the limits of this paper. The formation of cells from dividing protoplasm is unthinkable without the intervention of fat; a conglomerate of cells is easily produced without this separating medium. Indeed, the distinguishing feature between caseation and softening, and suppuration, seems to be the absence of fat in the former. For this, and the poverty of blood-supply (not to forget impeded absorption by way of the lymphatics), produces the non-separation of the cell conglomerate; and, when the circulation is altogether strangulated, death of these elements and destruction by caseation, and removal by the intervention of softening. Whilst in suppuration, hyperæmia in the presence of fat produces a rapid multiplication of cells, *i.e.*, the formation of pus corpuscles. These are some of the thoughts which urged the necessity of the use of oil in this disease, cod-liver oil particularly, since it is one of the most easily digested of fats, and perhaps because its natural combination with iodine makes it more capable of entering into intimate relation with the watery compounds of albumen in the tissues, thusly facilitating the interchange of fluids and the multiplication of cells, and the rejuvenation of the tissues. Finally, the use of iodide of potassium is again urged upon the profession as a remedy which may yet prove itself the chief anchor in this disease.

If the author's knowledge of the disease will entitle him to an innovation, a suggestion for the treatment of it, he will urge upon his fellows, particularly those connected with large hospitals, the trial of subcutaneous injections, in the neck particularly, of iodide of potassium and a small dose of iodide of mercury—say five grains of the former to gr. $\frac{1}{50}$ of the latter, highly diluted, and as often repeated as good judgment will warrant, as the most rational specific medication which, in the writer's opinion, it is possible to deduce from the nature of the disease. And, if to this be added a careful observation of all the suggestions which experience has sanctioned, together with a never-resting battle, by word and deed, against the spreading of the tubercle bacilli, we will succeed as never before in preventing and curing the disease.

Ashland, Nebraska, December, 1885.

A GRAVE CASE OF NERVOUS DYSPEPSIA CURED BY MASSAGE AND REST IN BED.

BY M. STAMM, M.D.,
OF FREMONT, OHIO.

It is not my intention to enter fully into the details of nervous dyspepsia, especially as its clinical picture is rather vague, and presents a complex of symptoms of the greatest variety. But the literature of recent years has furnished sufficient data to demonstrate that there are such affections of the stomach, which differ materially from the diseases of the chylopoietic system, otherwise described and classified, and which for the present may aptly be brought under the head of "nervous dyspepsia." Doubtless almost every busy practitioner has met with cases which presented such vague symptoms, and he vainly looked about for a description or a name which might approximately cover the picture presented. Our text-books of recent date make but short mention of it. The name "nervous dyspepsia" itself betrays the fact that we have no clear understanding of the pathological condition further than that the nervous system furnishes the prominent basis of these manifestations. Authorities are not agreed yet whether the changes are of local or central origin in the nervous system.

The cases thus far reported tend to show that some are due to functional disturbance of the pneumogastric nerves, and others to the ganglionic system. Another class of cases, again, may be referred to some abnormal condition of the central nervous system, especially cases due to mental influences, as care, anxiety, etc. From this we may see that it is no easy matter to form a diagnosis at the beginning, and that sometimes only the futility of the treatment instituted for the more common affections of the stomach and the obstinacy of the case will help to lead us on the right road. Worse than that, there are cases in which post-mortem examination alone will open our eyes, as a late report of a meeting of the London Medical Society graphically shows. Before that great body of highly scientific men Dr. Stephen Mackenzie, in a very instructive paper, mentioned two cases of apparent gastric disease, in which post-mortem examination failed to reveal any certain evidence of disease of the stomach, though the gastric symptoms were even definite enough to excite more than a suspicion of the presence of gastric ulcer. Indeed, one of the cases had been diagnosticated by more than one physician as a typical clinical case of the affection, whereas *post mortem* it was admitted to be a case of *apepsia nervosa*. Dr. Stretch Dowse, in the discussion, even ventured the opinion that perforating ulcers of the stomach were invariably of nervous dystrophic origin. Besides the difficulty of diagnosis, we may see that such cases even sometimes may terminate fatally, at least, that the treatment hitherto applied and recommended is not always followed by the desired effect. Such cases are generally looked upon as the *crux medicorum*, and not unfrequently go from physician to physician, and finally into the hands of quacks.

It may therefore not be out of place here to refer to a treatment which, in the case mentioned below,

had a surprisingly rapid effect. We all know what striking results the Weir Mitchell method of treatment will accomplish in some grave cases of hysteria and neurasthenia, especially when the state of nutrition has been very much lowered. And just the experience in a few such cases under my hands has led me to the remark, made about a year ago before the Sandusky County Medical Society, when the subject of nervous dyspepsia was brought up for discussion, that a similar treatment might result in much benefit in the latter affection. The rapid improvement and recovery in the following case, as well as the absence of any symptoms of hysteria and neurasthenia, may well justify the diagnosis of nervous dyspepsia,

Mrs. B., æt. about 27 years; married five years; had two children, neither of which she nursed herself. Her health up to one year and a half ago was excellent. About that time she suffered from intercostal neuralgia, which, however, was soon relieved by treating a cervical catarrh of the uterus. At times subsequently she would complain of a slight pain and pressure in the epigastrium; her appetite did not suffer any, and restriction of diet would soon relieve her of those feelings. About the first of August, 1885, the patient experienced shortly after meals a severe pain in the left epigastric region, which was frequently followed by vomiting. She would not admit any error in her diet, and it was impossible for me to find any other cause for her symptoms. Repeated examinations of the urine and sexual organs revealed nothing abnormal. She could bear very little food, and the various medicinal agents which her gastric symptoms and pain called for had merely a palliative effect, pain and vomiting would frequently recur. A case reported by Glax (Volkmann's *Klin. Vorträge*, 223), which exhausted all his therapeutical skill and change of climate without any avail, and finally, after he became dropsical, resorted in his despair to exclusive milk diet with the result of complete recovery, induced me to order such a diet in my case. It had the effect of lessening pain and vomiting; in fact, subdued it entirely, until she again tried more substantial food. This milk diet she kept up for about six weeks, which markedly improved her symptoms, although the emaciation made steady progress. Beef peptonoids, which also were tried, produced nausea and vomiting, I think principally owing to their looks and taste. Although Mrs. B., who in her health was always of a very patient nature, exhibited, *pari passu* with her gradual emaciation, some irritable disposition and signs of weakness, she never showed any real symptoms of hysteria or neurasthenia. About the beginning of October she visited her parents living in a larger city, and was there under medical care. During her visit her condition was only changed to the extent that her stomach could bear solid food and turned against milk, but her emaciation continued. Indeed, in December, when I saw her again, I found her reduced to a skeleton, and could hardly suppress the idea that a deeper organic change was at the bottom of her complaint. I accidentally dropped the remark in presence of patient that general massage and rest in bed, with proper feeding, might possibly give her some benefit, when she enthusiastically took up the idea, having heard of some splendid results by Weir Mitchell's plan of treatment in cases of hysteria and neurasthenia. I must confess her appearance at that time, the pain and vomiting having returned, did not inspire me with great hopes as to a cure. I therefore thought it prudent to propose only a trial of ten days.

As her nurse was not just ready to begin massage, I again put her on milk diet for about a week. During that time she only vomited once after having taken bread with her milk, her pain also had decreased considerably. The beginning of the treatment was fixed for December 24, and as she was free from any signs of hysteria and imbued with such implicit confidence in the result, the treatment was carried out at her own home, which in hysterical cases should absolutely never be undertaken.

In the first four days Mrs. B. complained of slight pains and pressure over her stomach; turning on her right side would especially increase her pain, but from that time on she experienced a feeling of unwonted well-being and enjoyed excellent sleep. Her bowels, which always before showed a tendency to constipation, began to move regularly, and after the sixth day she indulged in a bill of fare of rather liberal selection. As she reached the tenth day, the end of the proposed period of trial, she concluded to continue the treatment up to the end of six weeks, finding that the stomach made not the slightest remonstrance against those enormous quantities of food which Mitchell and Playfair urge upon their patients. In this manner she uninterruptedly followed the road to recovery, and although she has not accumulated such a mass of adipose tissue as some would perhaps lay up, she now presents a picture of perfect health.

Fremont, Ohio, February 22, 1886.

MEDICAL PROGRESS.

THORACIC ANEURISM TREATED BY THE INTRODUCTION OF STEEL WIRE INTO THE SAC.—At the meeting of the Royal Medical and Chirurgical Society, on February 23, DR. W. CAYLEY read the report of this case: The patient was a man, aged 48, who was admitted into the Middlesex Hospital on June 5, 1885. He had been suffering from symptoms of a thoracic aneurism, since November, 1884, but it was not till five days before his admission that a pulsating tumor made its appearance at the root of the neck, rising about three inches into the neck behind the right sterno-clavicular articulation. The patient was at first treated according to Tufnell's method, and given large doses of iodide of potassium. The tumor continued to increase in size, and it was evident that it must either soon burst externally, or extravasate among the tissues of the neck. On June 24, Mr Hulke introduced into the sac, through a fine canula, forty feet of steel wire. This caused no constitutional disturbance or local pain, and this portion of the aneurism became completely consolidated. Towards the middle of August, signs of the extension of the

intrathoracic portion of the aneurism—increasing dyspnœa, and severe paroxysmal cough—became more marked, and there was an increase of pulsation behind the sternum, and towards the left sterno-clavicular articulation. As it was evident that the aneurism must soon prove fatal from pressure on the trachea, it was determined to endeavor to consolidate the part of the sac producing this pressure. Accordingly, on September 10, Mr. Gould, in the absence of Mr. Hulke, introduced a canula just above the left sterno-clavicular articulation, directing the instrument obliquely towards the middle line, and introduced thirty-four feet nine inches of wire. No constitutional disturbance followed, but no relief was given to the symptoms, and the patient died in a paroxysm of dyspnœa on September 19. On *post mortem* examination, a large aneurism was found springing from the ascending part of the arch, and communicating with the vessel by a very large orifice; the whole of the upper portion was completely filled by a clot, embedded in which was the wire. The wall of the aneurismal sac, where it projected into the neck, consisted only of a little condensed connective tissue. The lower portion of the sac, near its origin from the aorta, caused compression and flattening of the trachea, just above its bifurcation. The first operation produced the desired result in preventing the imminent rupture of the aneurism. The size and connections of the sac rendered the second operation ineffectual.

The President congratulated Dr. Cayley and Mr. Hulke on their prolongation of the patient's life. He had seen all cases of large thoracic aneurism under his care die, except one, and that was in a man of 70, who died of old age thirteen years after clear signs of a large thoracic aneurism had shown themselves. The dangers from this treatment by the introduction of wire, he was inclined to think threefold; first from the local irritation induced, as had happened in Mr. Moore's case; secondly, from embolism, which might take place in the brain or kidneys; and, thirdly, from the ulceration in the sac, which might be caused by the end of the wire. Mr. R. Barwell could hardly think that the value of this method was as yet thoroughly determined. He believed that this was the ninth published case. The first was Mr. Moore's well known case; the second, a subclavian aneurism, into which Dr. Levis had introduced horsehair; the third, a case of popliteal aneurism, into which Mr. Bryant had also passed horsehair; the fourth, a case of innominate aneurism, under Dr. Rubio, of Madrid, treated by wire, which showed no change of symptoms during the first four days, but after that was lost sight of, and must be presumed to have died; the fifth and sixth cases, under Bacelli; the seventh, a brachial aneurism, under Van der Meulen, into which catgut was introduced, a proceeding which deserved considerable attention, as it avoided irritation; the eighth, a remarkable case of abdominal aneurism, under Professor Loreta, which was treated by abdominal section and separation of the aneurism from its surroundings, and the introduction of six and one-half feet of wire. The case did well till the ninety-second day, when the man died suddenly of rupture of the aneurism, just at its point of junction with its entrant artery. The aneurism, after death, was found to have in part consolidated, a result he was inclined to attribute to its manipulation, rather than to the small quantity of wire introduced. The ninth case was this now before the meeting, of Dr. Cayley's and Mr. Hulke's in which the man had survived the operation eighty-six days. The record could not be considered encouraging, and for his own part, he had not been tempted to adopt the operation.

Dr. J. Liston Paul, whilst surgeon to the General Hospital, at Madras, had had a case under his care in 1874, which he had treated in a somewhat similar manner. A native had been brought to him with a very large innominate aneurism, bulging forward on the left side of the sternum; into this he had passed fifteen feet of white cleansed horsehair. Slight pain and pricking over the tumor had followed. In three days the tumor had grown larger, and gave greater distress; on the fourth day the patient died in a convulsion. No search for emboli was made.

Mr. Bryant was very glad that this method of treatment was again brought under discussion, as he was disposed to think it applicable to more cases than was generally imagined. Of course it could only be thought of in those in which pressure, or manipulation, or ligature, was quite impossible. All such forms had but a faint chance; but in them the introduction of a foreign body should be attempted, if they were within reasonable reach. He preferred horsehair, or fishing gut, or catgut to wire; for the danger of irritation from wire was considerable. In his own case, to which Mr. Barwell had referred, the man was dying of ulcerative endocarditis; his treatment of the popliteal aneurism with horsehair, had consolidated it, and had so far lengthened life that it had prevented death from its rupture, which was imminent. In applying the treatment to aortic aneurism. the greatest difficulty would be in diagnosis; but, granted that that could be ascertained, he should consider the treatment most justifiable, and also in the case of abdominal aneurism.

Mr. Holmes said that he felt strong encouragement from Mr. Bryant's case, for the specimen showed clearly the progress that had been made towards cure; and Professor Loreta's case tended to the same conclusion. He could not agree with Mr. Barwell, that in that case the consolidation had been brought about by manipulation; a careful examination of the specimen showed the wire as the focus of the clot. The method had been originally suggested in a meeting of the Society, by Dr. Murray, of Newcastle, and he was under the impression that he had made some trials of it. To him they also owed the suggestion of the method of rapid compression under chloroform. There were many cases in which their choice was limited to two alternatives, treatment by electrolysis or by introduction of a foreign body. In his own experience, electrolysis had given purely negative results; others had done better with it, notably Ciniselli. In Mr. Moore's case, thirty-six yards of rather rigid wire had been used, and he had little doubt it was too much, and had set up inflammation; but he had no hesitation in recommending, with Mr. Bryant,

either horsehair or catgut, and of the two he rather preferred horsehair, as it does not melt away (which is the chief objection to catgut).

MR. HULKE disclaimed any credit for the paper, which was entirely due to Dr. Cayley, but was glad to find that the subject aroused attention. The President had suggested three dangers from the operation; the first was of local irritation. In Mr. Moore's case, the wire was of soft iron, which might easily have passed into the tube of the aorta, not tempered steel, as in Dr. Cayley's case. For his own part, he had still to learn any method of distinguishing whether the opening from the aneurism into the aorta was large or small. The steel wire which he had used had been carefully coiled beforehand, on a mandril half-an-inch in diameter, and he presumed that it would coil up again within the aneurismal sac. He passed the wire through a Southey's canula, in which were lateral holes; when sufficient had been passed, the end was cut with some effort, and slightly bent; this bent end caught in a hole in the canula, and when the canula was withdrawn, the end of the wire was left out, and pinned the coil of the wire in the aneurism to the chest-wall. That seemed at first unfortunate, as leading to some bleeding; but very possibly its counter-balancing advantages were greater, for it made it impossible that the coil of wire should fall from the aneurism into the main stream of the aorta. If horsehair or catgut were used, he could not feel sure how or where they would travel in the aneurism, nor could they be made certainly aseptic; catgut might be carefully treated, and yet give deadly results if it had been taken from animals dying of *milz-brand*. Of the possible methods of treatment, he considered the injection of ergotin in the neighborhood of the aneurism, as suggested by Von Langenbeck, as no longer worth discussion; the treatment of electrolysis in his own hands had proved positively disastrous; if the needles used were fine, they grew hot, and caused sloughing; if they were insulated, great force was needed to push them through the sac of the aneurism. The only remaining treatment was that by wire, and the arguments for that were in some cases strong.

DR. CAYLEY considered that the risk of embolism, which the President had suggested, was not serious; it had occurred in Mr. Moore's case, not from the treatment of the aneurism, but from the ulcerative endocarditis. He admitted that it was impossible to be certain in any diagnosis of a large opening into the artery from a small one; but, roughly speaking, a louder bruit indicated a narrower opening. He was able to assure Dr. O'Connor that there were no bubbles either in his case or in Mr. Moore's.—*British Medical Journal*, February, 27, 1886.

ACTION OF MERCURY ON THE BLOOD IN SECONDARY SYPHILIS AND IN ANÆMIA.—The influence of the salts of mercury on the circulation has been recently the subject of a most thorough and strictly scientific clinical study by DR. L. GAILLARD (*Archives Générale de Médecine*, November, 1885). The subjects on whom the first series of observations was instituted comprised:

1. Syphilitic patients without or with a slight anæmia.

2. Syphilitic individual with an advanced state of anæmia.

3. Syphilitic individual also with an advanced state of anæmia, but in a condition of mercurialization.

Gaillard's conclusions are as follows:

In the first group a diminution of the number of blood-globules and of the hæmoglobin could be observed in the course and at the end of the mercurial treatment.

In the second group a favorable result could rapidly be noted, both the number of blood-globules and the hæmoglobin increasing considerably and in a short time.

The subject of the third observation was not improved. The anæmia had no tendency to disappear, even after the effects of mercurialization had passed away. There was an evident intolerance of the drug, which must be borne in mind both regarding prognosis and the estimation of the therapeutic result.

Comparing these results, we see that the second category, representing syphilis and anæmia, profits most from the tonic and reparative action of mercury. It is hence in these patients that we can look upon mercury as a restoring and augmenting agent of both blood-globules and hæmoglobin. This refers only to the red blood-corpuscles; the white ones were nevertheless counted by Gaillard in each instance. Their number appears to diminish under the influence of mercury, as has also been as early as 1874 by Wilbouchewitz. [Still it appears hazardous to make any definite statement in this respect. We all know the instability and variance locally and numerically of the white blood-corpuscles. Remembering their abundance in persons affected with even slight suppurations, we would not be surprised to see them decreasing in a woman suffering with vaginitis after the cure of the local trouble.]

The second series of observations refers to the action of mercury on the blood in anæmia. The patient took ordinary doses of either the protoiodide of mercury or of the sublimate. The obtained results are comprised in the following theses:

1. The number of red blood-globules can decrease in anæmic persons under a mercurial treatment in the beginning without reattaining the original numeical status. More frequently, however, they increase in number progressively up to the fourteenth day, when a slight reduction takes place again.

2. The hæmoglobin increases daily and progressively up to the twenty-fourth day; then it is reduced toward its original figure. but surpassing it still, if the treatment lasts for several weeks.

3. The hæmoglobin increases more considerably in proportion than the red blood-corpuscles. It increases even when the latter diminish in number, so that mercury can be placed alongside of those metals which elaborate hæmoglobin.

4. The bodily weight increases nearly constantly under a mercurial treatment. Still, we are unable to establish an exact relation between this phenomenon and the condition of the blood.—*Therapeutic Gazette*, January 15, 1886.

THE
Journal of the American Medical Association.
PUBLISHED WEEKLY.

THE EDITOR OF THIS JOURNAL would be glad to receive any items of general interest in regard to local events, or matters that it is desirable to call to the attention of the profession. Letters written for publication or containing items of information should be accompanied by the writer's full name and address, although not necessarily to be published. All communications in regard to editorial work should be addressed to the Editor.

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SATURDAY, MARCH 27, 1886.

THE ETIOLOGY AND PREVENTION OF RABIES.

At the meeting of the New York County Medical Association on March 8, DR. HERMAN M. BIGGS, Instructor in the Carnegie Laboratory, who recently visited Paris to study Pasteur's methods with regard to rabies, read a paper on "The Etiology of Rabies and the Method of M. Pasteur for Its Prevention." It is not necessary to give in detail his argument for a specific origin of the disease. Suffice it to say that he expressed the opinion that the occasional spontaneous development of rabies in certain of the carnivora, as advocated by many distinguished veterinarians, must be entirely rejected in view of the facts recently demonstrated with relation to the germ diseases, and that we must accept absolutely the conclusion that this disease is maintained and spread solely by its contagious principle; there being no other causes in operation. Hence it is a matter of great importance that the fallacious ideas prevalent among the laity, and to no small extent among the profession, in regard to the influence of climate, season, hunger, thirst, food, pain, anger, ungratified sexual desire, etc., as predisposing or exciting causes, should be corrected. There are no more reasonable grounds for believing that any or all of these influences combined can bring about the development of a single case of rabies, than that unfavorable sanitary conditions can produce small-pox when the specific *contagium vivum* of that disease is not present. A wound or injury produced by the teeth of a non-rabid dog, one whose saliva does not contain the living organism (whatever may be its nature), which is the essential cause of the affection, can no more bring about the specific disease which we call rabies than can the wound made by a sterilized knife.

In regard to the nature of this *contagium vivum*, said Dr. Biggs, we have no absolute knowledge, except that it must be a living matter capable of reproduction and multiplication when transferred to the living organism, and there producing a specific disease manifested by varying symptoms in the different species of animals affected; of whose presence or virulence we can only know by the results produced when introduced into the blood of previously healthy animals. That it is exceedingly minute is shown by the difficulty experienced in its demonstration, and that it is a micro organism belonging to the schizomycetes is rendered probable by the similarity to other known pathogenic micro-organisms, as regards the mode of its transmission and development. Some of the conclusions arrived at by Pasteur in his earlier work have not been confirmed either by his own later investigations or those of other observers, and are in some respects opposed to the results recently reported. Unfortunately, too, in his last communications he has not discussed the question so much in detail as could be desired, nor has he given definitely the number or character of the experiments upon which his conclusions and the general principles drawn from them are based. The efficacy of Pasteur's present method was proven by inoculating fifty dogs, with success in every case in rendering the animals refractory to rabies. After having arrived at these results Pasteur felt justified, after consultation with MM. Vulpian and Grancher, in inoculating the boy Joseph Meister, who unexpectedly presented himself for treatment July 6, 1885. In order to determine the virulence of the virus used in this case two rabbits were each day inoculated in the usual manner with the same virus as that used for the boy. These inoculations showed that the cords used on July 6, 7, 8, 9 and 10 were not virulent, since they did not produce rabies in the rabbits inoculated on these days. The cords used on July 11, 12, 14, 15 and 16, however, were all virulent, and of a constantly increasing virulence. Rabies appeared after seven days of inoculation in the rabbits inoculated with the cords of July 15 and 16, after eight days in those of July 12 and 14, and after fifteen days in those of July 11. Notwithstanding that in the last inoculations virus was used which was far more virulent than that of rabid dogs which have contracted the disease in the ordinary way, these inoculations were followed by no symptoms whatever, and though more than six months have elapsed since the treatment, the boy still remains in good health.

The interpretation which Pasteur gave of the prophylaxis against rabies afforded by his method, by which an alteration in the virulence of rabic cords is

produced by the process of dessication, was, that the continuous contact of the dry air brings about a gradual diminution in the intensity of the virulence of the cords, until it finally becomes extinct; that the prophylactic method in its application depends for its efficiency upon the employment at first of a virus without appreciable activity, followed by a weak virus, and then by a more and more virulent form; that the diminution of the virulence of the cords is due to an impoverishment in *quantity* of the virus contained in the cords, and not in the impoverishment in *virulence*. Consequently, in the inoculations, the virus used is always identical as regards its virulence, and is variable only in respect to the quantity employed; so that the condition refractory to rabies follows from the employment of very small, but constantly increasing, quantities of virulence. This interpretation of the method of action of the virus, said Dr. Biggs, is the more interesting, inasmuch as a new and quite different principle is involved from that obtaining in the vaccine for small-pox or the vaccines devised by Pasteur for the prevention of anthrax, chicken cholera and typhus in pigs. In small-pox we have a virus modified in character and virulence by its passage through another species of animal; in anthrax, chicken cholera and typhus in pigs we have a virus modified in respect to its virulence by the conditions of temperature to which it has been subjected during its developmental growth; and, finally, in rabies we have a prophylactic method dependent upon the employment of a virus always constant as regards its virulence, but used in very small and constantly increasing quantities.

As to the duration of the refractory condition (to rabies) after inoculation, Pasteur believes this to be not less than one year, probably considerably longer; but as yet no data are available on this point. With regard to the conclusions as to the accuracy of Pasteur's observations and methods, Dr. Biggs thinks that the strongest evidence of the efficacy of the methods for the prevention of rabies rests upon the results of the experiments upon dogs. If Pasteur had been able not only to render fifty dogs refractory, without a single failure, but had also prevented the development of rabies in a larger number after they had been bitten—both of which he asserts he has positively accomplished—then the question as to the prevention in human beings is only as to the method of application. Dogs are far more susceptible and liable to the disease than human beings, and the character of the disease being evidently the same in both, if dogs can be made refractory it is not assuming too much to conclude that the same is true of human beings. In concluding his paper Dr. Biggs frankly acknowledged that the evidence as to the efficacy of Pasteur's methods is not altogether satisfactory. But in the light of his former brilliant and valuable scientific work is it probable that he could have spent six years of study to rabies without getting some substantial result? Or does it seem probable that he has been dealing all these years with some form of septicæmia, as has been suggested; laboring under the delusion in the meanwhile that it was rabies? This I cannot believe; and if it is not true, then Pasteur's conclusions must be in the main correct, for certainly no one will question his honesty."

As to the spinal marrow being employed as the agent for transmitting the virus, Dr. Janeway said, in the discussion of the paper, that it seemed to him entirely appropriate: the manifestations of the disease showed that the germ of the disease was in the spinal cord and medulla oblongata as well as in the saliva; or, if this was not the case, that it circulates as a specific poison in the blood and acts as an irritant upon the nervous centres. He thought that there was no reason to doubt that dogs had been made refractory to rabies by Pasteur's method, while in human subjects it had at least proved harmless in more than three hundred cases.

It is an indisputable fact that Pasteur has for years been experimenting with a disease which almost without exception runs the same course and produces the same symptoms; the period of incubation in rabbits being always about fifteen days. We must entirely agree with Dr. Biggs that if, as some assert, this affection be a form of septicæmia, if must be an entirely new form, since no variety of it is known which has a period of incubation in rabbits of fifteen days. After cultivation the inoculation in rabbits is seven days, and in dogs eight or nine days. In regard to the suggestion of Dr. Flint, Jr., that control experiments should have been made, such as the injection of bouillon subcutaneously, to which no spinal marrow was added, Dr. Biggs said that inoculations had been made subcutaneously and into the veins, instead of under the dura mater after trephining; the only difference that had been noted in the result was, that the period of inoculation was longer under these circumstances. The reason why Pasteur adopted the method which he now employs was because he found, after many experiments, that with it the period of inoculation is shorter, and the process more certain, than when any other method is used. Pasteur has inoculated dogs which were placed in cages with and had been bitten by rabid dogs; and *in not a single instance* did rabies result. These experiments

have been frequently tried under the supervision of the official commission appointed to investigate the matter; and this evidence is of the greatest importance in establishing Pasteur's claims as to the efficacy of prophylactic inoculation of rabies.

"ACADEMIC STUDIES IN RELATION TO MEDICINE."

In the department of Domestic Correspondence in this issue of THE JOURNAL will be found a letter on this subject by DR. R. LOWRY SIBBET, of Carlisle, Pa. It will be remembered that Dr. Sibbet read a paper at the last meeting of the American Academy of Medicine entitled "The Study of Medicine as a Means of Education," which was editorially noticed in THE JOURNAL of January 9. The present letter from Dr. Sibbet must be taken, it seems, as a reply to our editorial notice of his paper.

It is an evidently untenable proposition to say that *any* educated man, with large experience in any direction, is in a position to tell a boy what studies he should pursue as preliminary to the study of medicine. The President of Yale College is an educated man, and one of large experience; but surely he is no more fitted to speak dictatorially on this subject than many a well-read, clear-headed physician who sees reflected in himself the evils and mistakes of the ordinary classical course. "An educated man" is a relative term, with nothing fixed or definite in it. We cannot reason correctly unless we know the data from and upon which we reason. It is incorrect to say that one who has not pursued a study (such as mathematics) for a series of years is not in a position to judge of its relative value as a means of education. In view of the statements made in his paper before the American Academy of Medicine, Dr. Sibbet's use of the words "mathematics" and "languages" is entirely too broad for this discussion. We showed in our editorial that there is a great difference between ancient and modern mathematics; and it is useless to repeat what was then said. Hence we cannot argue the matter when our correspondent fails altogether to define his position, which is entirely different, as we shall show, from that taken in his first paper. In that paper he mentions only two languages, Latin and Greek. In point of training value one language has no advantage over another. It is a very simple matter to define and specify the mental processes involved in studying different subjects, and this is all that is required in determining the training value of any subject or study. To deny this is to confound the machinery of thought with thought itself, to mislead ourselves and make no progress, and to obscure the whole subject in a verbose fog of unwarranted assumptions.

In the propositions stated under (1) Dr. Sibbet argues entirely beyond the question at issue. No one has proposed that medical men, or those of any other profession, should regulate education or the curricula of colleges. He asserts in his first article that a man *must* be thoroughly grounded in Latin, Greek and mathematics in order to be fitted for the study of medicine. We denied that those studies are absolutely essential for a preliminary education for medicine, or for training the mind, intellect or faculties (or whatever we choose to say), and we *gave our reasons* for the denial. Dr. Sibbet has not shown that our reasons are incorrect, nor has he asserted that we reasoned from insufficient data. He adduced no proofs of his assertion, but evidently, as is so often the case, the assertion was made on the authority of that long list of classicists who have said the same thing over and over over again without bringing forward one iota of proof, or giving any reasons for it that would stand analysis. In his first article he did not refer to the value of scientific education (academic) or to science at all, except so much "as is usually taught in respectable classical colleges"—which means, usually, none at all. Why then does he now speak of "elective courses," which, as would be inferred from his article, he does not recognize as proper courses of study? It is seen that he has now changed his base of argument almost entirely, as he really acknowledges that a youth may, after all, be educated without being forced through the classical course. It is a mistake to say that the medical profession in the United States has ignored every standard, academic and medical, and in proof we may point to the Seventh Annual Report of the Illinois State Board of Health.

His answer to the question, Where should a student who desires a good education prepare for the study of medicine? is: in a classical, scientific or literary institution. How does this accord with what he says in his first article: that the student (before studying medicine) has "studied mathematics, languages, rhetoric, mental and moral philosophy, and as much chemistry, geology and astronomy as is usually taught in respectable *classical* schools. . . . . To all such we would say that they have laid a solid foundation upon which to add professional studies. Their education has been in all directions; not merely one class of mental faculties, but all have been exercised and disciplined." If an education *in all directions* can be obtained in a classical school, it must be evident that such cannot be the case in any other school,

since the methods in the different schools are essentially different. He has thus placed himself in the anomalous position of recommending schools which, as he has shown, he considers objectionable. But the truth is, there is no system of education which will educate in all directions. He evades the question, which he asks, What shall a student, who desires a good education, study until he is capable of deciding what he should do in life? His statement, under this question, that educated men in all countries and for many centuries have presented and advocated a combined classical, scientific, and literary course of study as the very best that can be devised for young men who desire to enter the learned professions, is singularly inaccurate. Three centuries are not *many*; and that is a liberal allowance for the age of our classical system. In the statutes of the University of Paris of 1600 the studies in arts are defined as Latin, Greek, Aristotle's Philosophy, and Euclid. Scientific and literary studies had no part or lot in the education of that period. Inasmuch as our science of to-day can scarcely be said to be one century old, it is manifestly absurd to say that it has been advocated for many centuries. It is not less absurd to hold that the preliminary education for the medicine of one-hundred years ago will fulfil the requirements of to-day.

It should be remembered that in speaking of the study of science we do not refer to getting lessons from text-books, but to the study of it in the laboratory. Canned science—that obtained from books—is not wholesome brain food. It is unreasonable to talk of special schools for the preliminary study of a particular profession. There is no need for them if instructors would study the pupils instead of making the pupils study them. The single fact that the elementary principles of chemistry and physics are "taught" in the medical schools is conclusive proof that they are not properly taught in the academic schools. On this subject we have already expressed our opinion in THE JOURNAL of May 2 and June 6, 1885. In this connection we take pleasure in referring to Dr. A. L. Gihon's Presidential Address before the American Academy of Medicine (at the same meeting at which Dr. Sibbet's paper was read), the more so because he plainly shows that he does not agree with some members of the Academy in thinking that a boy's mind must be matured on Latin, Greek, mathematics and canned science.

Finally, it may not be amiss to compare Dr. Sibbett's letter in this issue of THE JOURNAL with some of the statements in his first paper, as in this way we may more easily see how high a valuation he sets upon science and scientific schools in education: "The acquisition of knowledge and mental discipline are therefore dependent upon certain conditions. . . . No one would think of dispensing with books, instructors and institutions." Books may be and often are dispensed with in teaching science. The quotation can therefore refer only to *classical* education. In the requirements for admission to medical colleges, he tells us, "classical studies are generally treated with indifference." We know that scientific studies are equally disregarded; why is not the fact mentioned? Again, "Shall we tell it in New York that the oldest university in America does not require a knowledge of the five declensions of Latin nouns for matriculation in her medical department?" That is certainly not so disgraceful as not requiring a knowledge of the elements of natural science. "Another portion of our profession—possibly one-fourth—have advanced as far in algebra as quadratic equations, and in geometry as the *pons asinorum*. . . . In Latin they have reached the Bucolics of Virgil, or the Jugurthine War by Sallust, and in Greek the third book of Xenophon's Anabasis. Of rhetoric, logic, mental and moral philosophy they know nothing." From this it seems that he does not consider scientific principles as of any importance whatever. Again, "But what shall we say of the remaining half of our profession? . . . . They have never even glanced at the academic studies referred to, and consequently they are without the mental discipline and training which these studies always bring. . . . The student must become an active agent, not merely a passive one. He must work out problems in mathematics for himself, if he has not done so; he must translate foreign languages into good English, if he has not done so; he must prepare an original essay every week for four or five years, before he is properly prepared to study medicine." It seems absolutely useless to talk of scientific schools and scientific education until the papers from which these quotations are taken are retracted; and until this is done we beg to take leave of the subject.

INTERNATIONAL MEDICAL CONGRESS OF 1887. PROGRESS OF ORGANIZATION.

The statement in some recent medical journals to the effect that the progress of organization for the International Medical Congress of 1887 "seemed to have come to a standstill," is by no means correct, as the following facts will show:

Soon after the final meeting of the General Committee on Organization appointed by the American Medical Association in September last, at which the said Committee adopted entirely satisfactory Rules

for the Congress, selected the general officers, and chief officers of the several Sections, thereby developing under their Rule Ten an independent Executive Committee of the Congress, authorized to take charge of the entire interests and further management of the organization, the said Executive Committee held a full meeting in November and organized for their work. The President and Secretary-General were instructed to issue a preliminary circular giving the general officers selected and the Executive Committee, together with the Rules in English, French, and German, and distribute the same both in Europe and America. At the same meeting sub-committees were appointed on all the more important items of unfinished business, to report at another meeting of the Executive Committee to be held in a few weeks from this time. These sub-committees have been quietly but diligently prosecuting their work so successfuly that at the next Executive Committee meeting there will be neither difficulty nor delay in filling all vacancies needed to make the preliminary organization of the Congress complete, and be ready to proceed directly with the preparation of the details for the final programme for the work of every department of the Congress. The local committee of reception and arrangements at Washington, with Dr. A. Y. P. Garnett for Chairman, has increased its members to fifty, acting in harmony, and for more efficiency in execution, divided into seven sub-committees, with each its allotted part of the work to be done.

This is a guarantee that every possible arrangement will be made for the interests and comfort of those who may attend the Congress from any part of the world, and that active efforts will be made to secure the lowest rates of travel on ocean steamers and railroads for those coming from other countries, and in regard to which timely notice will be given. With the aid of the Associate Secretary-General in New York the circulars anouncing the Preliminary Organization and Rules of the Congress were freely distributed in foreign countries, and its reception was immediately followed by cordial responses, which have continued to be received almost daily at the office of the Secretary-General from many of the countries of Europe, more especially from Great Britain, France, Germany and Austria, giving assurances of personal attendance, promising communications for certain Sections, and asking for further information. Instead, therefore, of a "standstill," the work of preparation for the Congress of 1887 is progressing rapidly and successfully towards completion. We have deemed it proper to make the foregoing statement to prevent misapprehensions, and for the general information of the medical profession at home and abroad.

SOCIETY PROCEEDINGS.

CHICAGO MEDICAL SOCIETY.

Stated Meeting, March 15th, 1886.

THE PRESIDENT, C. T. PARKES, M.D., IN THE CHAIR.

DR. ROBERT H. BABCOCK made remarks on

TWO CASES OF MITRAL STENOSIS, WITH PRESENTATION OF THE PATIENTS.

DR. BABCOCK said that he did not intend to give any discussion of mitral stenosis, but merely desired to present a few points of interest in connection with these patients. Mitral obstruction is due to a fusing together of the valves so that they project into the ventricle in the form of a funnel, which, according to many authors, is the most frequent form of stenosis; according to others, the obstruction is due to a septum-like valve stretching across the opening, and called the diaphragmatic valve. Owing to the obstruction to the flow of blood from the auricle into the ventricle a murmur is produced which is rolling, or blubbering in character, and, occurring during the auricular systole and previous to the systole of the ventricle, is called pre-systolic or auriculo-systolic. This murmur has been graphically represented by Balfour as in some cases resembling the sound of voot-rrrb, the final t or b being the sudden, abrupt first sound produced by the ventricular systole.

In some cases no pre-systolic murmur, correctly speaking, is heard, only a diastolic murmur which, being loudest at the apex in the mitral area, is a mitral diastolic murmur. This murmur should be differentiated from the diastolic murmur of aortic regurgitation which, in certain rare cases described by Balthazar Foster, has its maximum of intensity in the mitral area and not at the centre of the sternum.

In the two cases Dr. Babcock presented the murmurs differed from each other in character, in the one case the murmur being very distinctly represented by the letters voot, in the other by rrrb. The doctor was disappointed at the last moment by his inability to present a third patient, a man in whom the stenosis was indicated by a mitral diastolic murmur which followed an impure first sound of heart, and both the impure first sound and the diastolic murmur were in this case audible at the lower angle of the left scapula. The propagation of the diastolic murmur so far to the left is very unusual; indeed, the production of the mitral diastolic murmur is itself very rare, and is probably due to the fact that a rush of blood from the left auricle to the ventricle occurs with greater force at the beginning of the ventricular diastole than during the auricular systole, the auricle being dilated rather than hypertrophied, according to Sansom. The cases were then examined by members of the Society.

DR. E. C. DUDLEY reported a

UNIQUE CASE OF VESICO-VAGINAL FISTULA,

in which the entire vesico-vaginal septum, the vaginal portion of the cervix, and anterior wall of the cervix to the internal os had sloughed away, leaving no

bladder tissue between the inner extremities of the urethra and the points at which the vesico-uterine ligaments connect the bladder with the uterus. The only operation which seemed possible was to unite the posterior wall of the cervix uteri with the neck of the bladder. This would turn the uterus into the bladder and necessitate menstruation through the urethra. The anterior wall of the uterus could not be approximated to the neck of the bladder, but it was found, on further examination, that the mucous membrane of the bladder, if caught with the tenaculum about an inch in front of the uterus, could be drawn to the neck of the bladder and held without undue traction. The operator therefore undertook to close the fistula in this way by denuding a strip of the mucous membrane of the bladder from side to side an inch in front of the uterus, and thus he utilized that portion of the bladder between the line of denudation and the uterus, and made it a substitute for the lost anterior wall of the cervix and vesico-vaginal septum. Twenty-two silver wire sutures were employed after Sims's method. Union by first intention followed, notwithstanding the failure of the nurse the third day to keep the catheter *in situ*, which allowed several ounces of the urine to accumulate in the bladder. Notwithstanding the decrease in the size of the bladder necessitated by the operation, the patient experiences no difficulty in retaining the urine all night. The operator is not aware that another case of this kind has been previously reported.

THE PRESIDENT asked if it was not possible that some portion of the upper wall of the vagina was drawn upwards and backwards by the bladder, and what was taken to be a continuous wall of the bladder might be a part of the vagina. He had seen a case where there was a large laceration into the bladder, and the opening seemed one cavity with continuous walls, but the flap thrown backwards was post-vaginal, and it was found that this flap could be drawn up and into its position.

DR. E. C. DUDLEY, in answer to questions, said that the loss of tissue at the base of the bladder from sloughing differs from that by incision. In the latter case the ureters would perhaps be included in the excised tissue, but it is seldom that a slough of the base of the bladder in vesico-vaginal fistula, however extensive, destroys the connection between the bladder and kidneys. Even if the points through which the ureters penetrate the mucous coat of the bladder be lost, it is yet possible their openings into the bladder may be preserved, because the ureters penetrate the muscular coats nearly an inch from their normal points of opening through the mucous coats of the bladder, and run obliquely between the two coats for a distance of nearly an inch. In this case, as in many cases of loss of entire base of bladder reported by Emmet, the openings of the ureter were on either side, at the very margins of the fistulous opening. The operation was performed at Morton, Ill., in the presence of Dr. Harris, of that place, and Dr. Mansfield, of Metamora. Dr. Parkes's surmises with reference to the vaginal wall could hardly be correct, because this, together with the anterior wall of the cervix uteri up to the internal os, had sloughed away. His surmise might be correct with reference to certain tissues between the bladder and cervix uteri which might have retracted and become adherent by inflammation, so as actually to form a portion of the bladder wall. Moreover, there is always a very decided difference in color and appearance between vaginal and bladder membrane, and the membrane in this case was to all appearance like that of the rest of the bladder, and to the touch gave the sensation of a thin wall. Dr. Baker, of Boston, reports a case similar to this in that he introduced sutures into the bladder tissue, but so close to the cervix uteri as not to draw down any portion of the interior of the bladder, to be used as material in place of the lost vaginal wall.

DR. W. L. AXFORD reported

A CASE OF REMOVAL OF THE ENTIRE LOWER JAW THROUGH THE MOUTH.

Harry T., aged 5. Admitted to St. Joseph's Orphan Asylum in November, 1885. It was noticed that his mouth was frequently swollen and sore. Child very much emaciated. In January, 1886, he had measles. Tedious convalescence followed. Came under observation about February 1. Weak and thin. Lower part of face very much swollen. Breath offensive. Symphysis of jaw bare. Could not examine further at this time. Pulse 120 to 130. Put the child on supporting treatment, hoping to get him in condition for an operation. No improvement at the end of two weeks. February 16 the patient was anæsthetized and the mouth explored. Found the jaw on either side stripped of its periosteum back to the masseters. Determined to attempt removal through the mouth, as any cutting operation involving the loss of much blood would have been fatal at once. Divided the jaw on either side of the symphysis with bone pliers and thus removed a large portion of the body. Seizing the remaining pieces with sequestrum forceps and making moderate traction, they were easily enucleated by the index finger of the left hand. Not more than a tablespoonful of blood was lost. Patient rallied well. Some reaction on second day. On third day the pulse had dropped to 116, and with exception of a swollen parotid on the left side, the child was in better condition than before the operation; so much so that a recovery was confidently predicted. A severe attack of diarrhœa occurring on the morning of the fourth day was followed by death in thirty-six hours.

DR. ARNOLD P. GILMORE exhibited a patient on whom he had performed an operation for

SYMBLEPHARON OF THE LOWER LID,

due to a burn by molten iron, and in which three plastic operations had been unsuccessfully performed. Nine months previously the entire lower lid, from external to internal canthus, was adherent to the eyeball, covering almost the entire cornea. This triangular-shaped tissue was covered by a pale membrane of cicatricial tissue. The operator first detached the lower lid and transplanted the conjunctiva of a rabbit. For six weeks the operation was apparently successful, but after an absence of two months from

the city he found the lid was again becoming adherent. Six weeks previously Dr. Gilmore made a thorough dissection, freeing the lid and making a deep cul-de-sac, leaving the upper half of the eyeball covered by mucous membrane and the lower half bare. A semicircular band of conjunctiva, one-third inch wide, close to the cornea above, leaving a bridge of tissue at each end. This band was dropped into the cul-de-sac below and carefully stitched to the ball. A semi-circular plate of silver long enough to fill the space between the external and internal canthi, with two holes at the circumference, one-half inch apart, threaded with silver wire, was dropped into the cul-de-sac to prevent adhesions, and fastened by bringing the wires through upon the face and fastening them by small lead plates and perforated shot. For this operation Dr. Gilmore claimed priority. The object of the operation was neither to improve the appearance of the eye nor to restore vision, but to relieve the irritation of the other eye, by allowing coördinate movements of the two eyes. There was enough clear cornea left to make an artificial pupil in case the patient ever lost his well eye. There was little reaction, and at no time much pus, while the well eye has grown stronger in spite of the presence of the plate.

Dr. Tilley thanked Dr. Gilmore for showing this case, but thought that if the Doctor were to go out of town again for two months as in the first instance, he would find at the expiration of that time the conditions relatively very much the same as on his return after the first operation. He thought there was little fundamental advantage likely to be associated with the operation, as he thought that in a short time the wire and plate would cause a certain amount of atrophy of the intervening tissue and the plate be forced up out of position, making the operation of no avail. If he was so unfortunate personally as to be placed in a similar position, he would have his eye enucleated.

Dr. E. L. Holmes thought it unwise to say that a certain thing could not possibly be accomplished, but he had been through the experience of putting in plates, and seeing it done, and never saw one permanently successful. It is different with a very narrow symblepharon in which the globe and eyelid are grown together, where by dissection and transplanting the mucous membrane excellent results may be attained. He thought the plate would irritate the cicatricial tissue and cause it to be very much thickened, and after a few months, or weeks even, when everything is removed, there will be the same tendency to creep over the cornea and make adhesions with a broad union. He thought it absolutely impossible to get an artificial eye to fit. A very small eye might be used and temporarily make it appear that the patient was better off, but that small eye will often irritate and cause the cicatrix to increase.

The President thought this case one of the same category that is so troublesome to the general surgeon, the improvement of deformities from cicatrices of all kinds, in which relief comes only in the way that Dr. Dudley has applied in gynecology, after the divided cicatrix has been separated as widely as possible, by drawing together the healthy skin or tissue between the two ends of the divided cicatrix. This method has long been in use in general surgery. So far as his experience went the application of any foreign body between these divided surfaces has never been followed by success, so far as prevention of contraction goes.

The President presented an

ENCAPSULATED SARCOMA OF THE THIGH.

It had been in alcohol for some time, and was reduced about one-third in size. It had grown the full extent shown in three months, and was removed from an old lady aged 69. It was found growing upon the posterior part of the upper portion of the thigh; was a firm, smooth tumor to the touch, and as far as external manipulations determined could not be distinguished positively from other parts of the surrounding tissues. He could not determine whether it was or not attached to the bone, but from external appearances it was diagnosticated to be of a malignant type. The external surface was crossed by a large number of varicose veins. After removal it was shown to be a sarcoma. The interesting point was the rapidity of its growth. He thought it a singular coincidence that about a year previous he had removed a similar tumor from the upper portion of the left arm of an old man of 72 years, which had also grown to the full size in three months. Upon exposing the tumor a perfect capsule was reached, and it was easily enucleated from its bed.

GYNÆCOLOGICAL SOCIETY OF BOSTON.

Annual Meeting, January 14, 1886.

The President, Henry O. Marcy, M.D., in the Chair.

H. J. Harriman, M.D., Secretary.

The following

NEW MEMBERS

were elected: Drs. Helen L. Betts, Emily Pagelsen, Grace Wolcott, Lena V. Ingraham, N. O. B. Wingate, A. E. McDonald, and J. E. Kelley.

The following were elected

OFFICERS FOR THE ENSUING YEAR:

President, Dr. H. O. Marcy; Vice-President, Dr. H. C. White; Secretary, Dr. H. J. Harriman; Treasurer, Dr. W. Symington Brown; Pathological Committee, Drs. E. W. Cushing, A. P. Weeks, and H. O. Marcy; Nominating Committee, Drs. H. M. Field, E. C. Keller, and C. W. Stevens.

Stated Meeting, February 11, 1886.

The President, Henry O. Marcy, M.D., in the Chair.

Dr. A. L. Norris read a paper on

CERTAIN PATHOLOGICAL CONDITIONS INCIDENT TO THE FŒTUS;

illustrated by a series of cases.

Case 1.—Mrs. S., married, age 26 years. Mother of one child. Last menstruation, April 5, 1885.

Had no symptom of pregnancy except cessation of menses. During previous pregnancy had suffered from nausea and vomiting. Was examined August 1, 1885, and appeared to be about three months pregnant. She did not think that she could be pregnant, but decided to wean her child, which was 13 months old. She was examined again on Oct. 22. Had appearance of being pregnant at four months. No motion; no fœtal heart-sounds. Abdomen was not much larger than at August examination. In the meantime she had been examined by other physicians, who thought a uterine or ovarian tumor to exist. January 4, 1886, the abdomen seemed less in size than at October visit. On January 6 she was delivered of a 4½ months fœtus which was mummified and enclosed in membranes. Fœtus, membranes and placenta were extracted in one mass nine months from date of last menstruation. The usual amount of hæmorrhage followed delivery. No odor. Convalescense normal.

Case 2 was similar to case 1. A fœtus was carried five months without symptoms other than cessation of growth. Cases 3 and 4 were cases of triplets. In case 3 the mother had had eight single births previously. One boy and two girls were born at a quick and normal labor. Mother nursed the three children until they were several weeks old. One girl and the the boy died when six weeks of age. The other girl still lives and is in good health. In case 4 a primapara was delivered of two healthy girls and a still-born boy. The fœtus of the boy was of three months development, and was mummified. Both girls are alive and well.

Case 5 was one of spontaneous amputation *in utero* of both arms at the shoulder. During the last four months of gestation the mother suffered from intractable chronic diarrhœa and from vomiting. Boy is still alive and well, and uses his toes with almost as much facility as other boys use their fingers.

DR. H. M. FIELD, in discussing Case 1, asked Dr. Norris if he regarded pregnancy as a bar to further nursing. He called attention to a series of articles by Dr. Dolan on the influence of drugs upon lactation, in which that writer takes exception to the popular opinion upon this point. He found that the milk of pregnant women was not impoverished, and concluded from this fact that pregnancy was not a contra-indication to further nursing in a majority of cases. In France the laws forbid a pregnant woman's acting as wet-nurse, upon the ground that the milk of such women is inferior both in quantity and quality, and thus the health of the nursing child is in danger.

DR. NORRIS, in reply to Dr. Field, said that in his opinion a pregnant woman should not nurse a child. Abortion was likely to follow such a practice, and the health of the child was endangered. Children nursed by pregnant women were invariably ill-nourished, as proven by loss of color and flesh.

DR. WARNER took exception to Dr. Norris's conclusions in regard to his first case. Dr. Warner thought it far more probable that the fœtus had continued to live up to the time of its expulsion, but through some vice of nutrition had ceased to grow, than that it had died during the early months of gestation and been retained until the ninth month.

DR. A. P. CLARKE described several cases which were similar to Dr. Norris's first case.

Case 1.—Mrs. B., 38 years old; multipara. Miscarried three times. Menses regular until last August, when they disappeared. She suspected she was pregnant, though no morning sickness or other usual symptoms of pregnancy appeared. A fœtus was expelled and the placenta found to be fatty. Amount of amniotic fluid was small. Fœtus was buff-colored, somewhat flattened, and measured seven inches in length.

Case 2.—Mrs. W., 36; multipara. When first seen was suffering from severe uterine hæmorrhage. Vaginal examination revealed a cyst presenting, which was ruptured by the examining finger. A blighted fœtus, three inches long, which had undergone mummification, was removed. Placenta had been attached until time of expulsion, and fresh hæmorrhage followed its removal. Menses had been absent for eight months preceding the expulsion of the fœtus. At first patient had suffered from morning sickness, but during last five or six months had had no such trouble. The breasts had increased somewhat in size. Patient had grown stouter, but abdomen had not increased in size during later months of gestation.

Case 3.—Mrs. W., 34; multipara. Labor normal and female child born at term. Child well developed. On the removal of the placenta a flattened and mummified fœtus was expelled. Was of a buff-color, six inches in length, enclosed in membranes.

In reply to Dr. Warner, Dr. Clarke said that mummification usually takes place in cases where the fœtus dies, the fluid elements of the body are reabsorbed, and the membranes are unruptured. If, however, the membranes are ruptured and the air enters, putrefaction takes place.

DR. E. W. CUSHING said that there was nothing impossible about a fœtus being carried for months after its death, as claimed by Dr. Norris. There are cases on record which seem to prove it beyond a doubt.

DR. M. D. CHURCH described the case of Mrs. M., who, at nearly full term, received a fright and fall in a horse-car on March 26, 1883. Was delivered of a still-born fœtus, twenty-two inches long and fully developed. Funis was drawn tightly about the neck of the child twice. The epidermis slipped on all parts of the body. He raised the following questions: 1st. How long must a fœtus be dead *in utero* before the skin will slip? 2d. Was the tight funis the cause of death? 3d. Was the fall in the horse-car the cause of the tightening of the cord?

DR. KELLER described the following case: Mrs. S., 35. Five children at term. Miscarried five times on account of lacerated cervix. The fifth time had been pregnant eleven weeks when she had "a profuse flow of blood with clots." She was carefully examined by a physician, who concluded that the uterus was empty. The flow continued nine weeks when an ovum entire with fœtus 1½ inches long was expelled. No odor. The product of conception seemed perfectly healthy when cast off.

Dr. Helen Betts, in referring to the question of a woman nursing when pregnant, thought that few women were able to nurse a baby and carry a fœtus at the same time without injury either to themselves or to those in their charge. In most cases it seemed wise to withdraw a part of the burden by directing that the child be weaned.

The President, in commenting upon the specimen presented by Dr. Norris, said it was one not only of exceptional interest, but represented a whole series of placental changes which, until recently, had been but little understood. The late distinguished Professor G. B. Ercolani, of Bologna, rendered a service to the profession of incalculable value in his studies of placental development, and his demonstrations, after long and patient research in the large field of comparative as well as human anatomy, are of the first importance to science. He shows conclusively that fœtal alimentation in all mammals is carried on by a single, universal law of physiological modality, that the fœtal villus is simply a means of absorption of nutritive material, which is, in all cases, furnished by a glandular, secretive organ of maternal development. The expression of this anatomical type is of the widest variety, and in woman, in the fully developed placenta, reaches a complexity of form confusing in the highest degree. The chorial villi, which during the earlier months of pregnancy are developed over the entire surface of the chorion, are composed of two parts, internal and external. The first, which was called by Robin chorial tissue, and by Virchow mucous or myxoma, is in direct contact with the chorion; the latter, which consists of an epithelial envelope, entirely surrounds it. The villi, at the placental site, increase normally in number and volume. Elsewhere, they are arrested in their growth and disappear by atrophy and fatty degeneration. The decidua is, at first, a single bed of cells of new formation, and afterward proliferates to a remarkable degree at the placental site—the decidua serotina. The epithelial covering of the placental villi is perceived same time after they have been enveloped by the cells of the serotina, but disappear at an early period. Not seldom, in case of so-called mole pregnancy, the choroid villi, which should atrophy, increase in volume. Under these conditions the external epithelium does not atrophy, but proliferates, and has been mistaken for the internal surface of the epithelium furnished to the placental villi from the modified cells of the serotina. At the later stages of pregnancy, the examination of any remaining choroid villi shows them still covered with an epithelial layer, while the placental villi have lost this envelope and are formed only of the fundamental myxomatous tissue which encloses the fœtal vessels, and thus the external parenchyma of the villi is in contact with the internal epithelium of the surrounding glandular organs.

Pathological conditions, such as are represented in this specimen, where the fœtus has been for some time dead and the placenta has remained undisturbed in its attachments to the uterus, give confirmatory evidence of the truth of the demonstrations of Ercolani. I have frequently observed, microscopically, the close contact of the choriol villi with the epithelial covering of the maternal vessels when the placenta has been undisturbed in connection with the uterus for a considerable period after the death of the fœtus. The choriol villi have become shrunken, and are easily separated from the decidual sheath, the cells of which have undergone active proliferation. In one instance this was observed to have taken place and continued in a stage of active development, in a portion of a placenta removed three months after the expulsion of the fœtus.

Dr. Marcy was glad to have the attention of the Society called to this class of specimens. They are of interest from the practical side of observation and often involve danger to two lives. The pathological committee of the Society will gladly investigate and report upon any specimens which may be forwarded to them.

MEDICAL SOCIETY OF THE DISTRICT OF COLUMBIA.

Stated Meeting, February 17, 1886.

Vice-President, John B. Hamilton, M.D., in the Chair.

T. E. McArdle, M.D., Secretary.

Dr. C. V. Boarman read a paper on

JAUNDICE FOLLOWING CONCUSSION OF THE BRAIN.

On December 30, 1885, I was called to see J. B., age 30; married; a laborer at the Monument; who, whilst assisting at some mechanical work, lost his balance and fell a distance of thirty feet, striking upon his head, tearing the scalp from and exposing for several inches square the vertex of his skull, he also bruised and lacerated his forehead in several places. His body and limbs were also bruised. I found him at his home (to which he was brought in a carriage), in a sitting posture, apparently dazed; would answer questions in monosyllables after their repetition in a loud voice, was spitting blood and his nose bleeding.

I washed the semi-detached scalp in carbolized water and reapplied it in its proper place, holding it there by means of a stitch and adhesive plaster, ordered him to be put to bed at once and lotions applied to his face and other bruised parts. About six hours after a messenger came to my office to ask for something to relieve Mr. B.'s sick stomach, and also notified me that he had vomited about a pint of coagulated blood, and that there was still considerable retching and nausea. I at first thought that the above might be caused by compression, but as I gave the injury a very thorough examination at the time I saw it and discovered nothing of the kind, I concluded that it was due partly to the concussion, and also the blood which had run down the œsophagus from the posterior nares, as there was considerable epistaxis. I ordered ice, lime-water and milk, also some ingluvin, and after a few hours bromide potass. in solution, as there was considerable excitement, groaning and jactitation.

December 31. Mr. B. was quietly resting upon

his back in bed upon my arrival, perfectly conscious, no headache or sick stomach, but still spitting up blood. He stated that after vomiting copiously the night before, he felt much better, became perfectly conscious and passed a good night. The vomiting was no doubt as much dependent upon the blood which had run down his œsophagus as upon the concussion of the brain. I examined his wounds and found them doing well; very little suppuration, the scalp-wound inclined to unite. The entire head, face, and even the neck was very much swollen, looked like it was puffed up with air, but no redness or tenderness; in fact, no positive symptom of erysipelas. His sclerotics were congested and effused, the surrounding parts black from bruises. He complained very much of soreness about his knees and shoulders, which were no doubt struck by projecting beams during his descent.

January 1. Upon visiting Mr. B., to my astonishment I found him intensely jaundiced, his sclerotics stained, urine loaded with bile-pigment, and all other symptoms indicative of icterus. I never saw a person more changed in appearance; being in health a very tall, thin man, his features sharp and cheek-bones prominent, his present condition was such that he was hardly recognizable, as his face was terribly swollen and as yellow as saffron, the bruises and lacerations of course adding to the above abnormal appearance. I removed the stitch from the scalp-wound, which had united by first intention. His other injuries, notwithstanding his jaundiced condition, were doing well; he was still spitting up some blood, but not enough to cause any alarm. I ordered a mercurial purgative, after the action of which I placed him upon nit. muriatic acid dilut., at the same time, of course, regulating his diet, forbidding the fats, oils, etc. The purgative acted well, bringing away a large amount of black grumous matter, no doubt partially digested blood and fæces. He felt considerably relieved after his bowels acted, and left the bed.

January 3. His condition was very much improved, the jaundice rapidly disappearing, and the swollen and bruised parts resuming their normal condition. I did not see Mr. B. again until January 5, which was my last visit, as the jaundice had disappeared, and his various injuries were doing well, he was up and anxious to resume his work.

I look upon the above case as rare, as in the majority of cases the physician is able to trace the disease to obstructions either existing in the intestinal canal or common duct, whereas in the above the solution of the difficulty is hard, unless we accept the theory advanced by Murchison, that concussion of the brain produces jaundice by interfering with or deranging the normal metamorphosis of bile by nervous influences. Virchow mentions a form as hæmatogenic, that is, formed in the blood or taking its origin there. Ponfick assigns as a cause violent mental conditions, as for instance anger, etc. Murchison speaks of the case of a soldier who became jaundiced because he was restrained from wreaking his vengeance on a comrade who had angered him.

M. Rendu records the case of a woman aged 25 years, convalescent from puerperal peritonitis, who became jaundiced within three hours after an unsuccessful attempt to catheterize the bladder; the skin remained yellow for four days and the urine contained bile-pigment one day longer. Its sudden onset and rapid disappearance are in favor of its being of neurotic origin. Another case of interest having a remote origin or cause is mentioned by Dr. Boucher in the *London Lancet*, July, 1884: A child aged 2 years sat down in a pail of hot water in which there was dissolved a quantity of soda. The scalding was confined to the nates, on which there were two large blisters, they were treated by the usual methods. On the tenth day after, there was abdominal tenderness with pain, high temperature and diarrhœa, the urine contained bile-pigment, sclerotics and skin stained; in a word, a well-marked case of jaundice. It lasted three or four days, and then subsided. This case was no doubt owing to inflammation extending to the mucous membrane of the common duct, causing an obstruction to the outward flow of bile, which in turn was followed by jaundice.

It is frequently very difficult to assign a cause for jaundice, as its appearance at times is so eccentric; for instance, the jaundice of pyæmia, and those forms of the disease resulting from the narcotism of chloroform, and other forms of intoxication. It also occasionally complicates pneumonitis. I think the case just reported by me was due to neurotic influences, as its appearance and disappearance were so sudden. Besides, I examined his urine by the usual methods, and failed to find the biliary acid, which, according to Dr. G. Harley, is proof that no obstruction existed, as he adopted the theory which had been advanced, "that the liver manufactures the bile acids, while it merely excretes the bile-pigment."

DR. A. Y. P. GARNETT said that jaundice is often due to traumatism. Although he had no personal experience with the disease resulting from such a cause, yet the journals frequently contain reports of similar cases. Nervous influences operate in a great variety of ways. Some women suffer from jaundice during the period of the menses. Some reflex irritation traceable to the uterus or ovaries produces an icteroid condition which disappears with the cessation of the menstrual flow. We know that the presence of the infant seeking nourishment will produce an increase of maternal milk. The appearance of food causes a secretion of saliva. Not only quantitative but qualitative effects follow mental emotions. Passion has a deleterious influence on normal secretions. Fear had turned turned hair gray almost instantaneously. Dr. Garnett also spoke of jaundice following the bite of serpents.

DR. J. FORD THOMPSON had no experience with jaundice as a sequence of concussion of the brain. He had seen icterus follow injuries about the skull, and he had seen the yellow discoloration of pyæmia and septicæmia; but he did not consider this condition pathological jaundice. It is different from a jaundice caused by an impediment to the excretory ducts or by a catarrhal condition. In such cases there would be an absorption of bile after its manufacture by the liver. When we get to explaining things by nervous shock we do not get much satisfaction.

Dr. Boarman contended that jaundice may be a symptom of nervous derangement. In this case he found bile pigments and biliary salts in the urine. It looks, of course, as if an obstruction existed. The patient had headache and nausea, both symptomatic of jaundice. There was at no time even a suspicion of pyæmia or septicæmia. The pulse was slow; but that condition is common in jaundice. Murchison says that nervous influences can and do produce jaundice.

Dr. Garnett said that the metabolic activity of the hepatic cells in the production of bile is in direct proportion to the plus or minus degree of blood pressure in the liver. Any agent or cause disturbing the normal physiological equilibrium of blood pressure in that organ directly affects the secretion of bile. It has been shown that a section of the splanchnic nerves causes immediate dilatation of the hepatic veins, followed by a diminution of arterial blood pressure and increased flow of blood into the portal veins. It is easy then to conceive how readily causes disturbing the normal functions of the nerves supplying the liver, and especially the great sympathetic, whose filaments supply the vaso-motors of the hepatic arteries, may affect the secretion and diffusion of bile.

Dr. Boarman said that Budd concurred in Boerhaave's and Morgagni's idea, with the exception that no bile acid was found in the blood of the portal vein. Prof. Freidrich states that bile acid taken from the liver by blood of hepatic vein, and also absorbed from bowel, etc., undergoes in the blood a change. Bile acids converted by oxidation, a normal metamorphosis, into taurin, are found in healthy lungs, and taurin and pigment are voided in the urine.

Dr. A. F. A. King inquired whether the body was examined all over? If the discoloration was general? If there was bile in the stools? He thought the discoloration resulted probably from bruising. He said that it was important to determine whether a yellow coloration of the surfaces is really due to the principle of the bile, and mentioned the true jaundice of the new-born, and its spurious counterpart.

Dr. Boarman had examined the body generally but not the abdomen. The urine, he repeated, contained the biliary salts, the fæces were scanty, and the color of dog's dung.

Dr. H. D. Fry asked on what symptoms the diagnosis of concussion of the brain were based?

Dr. Boarman repeated the symptoms mentioned in his paper.

Dr. Fry thought it was important to have examined the abdomen for possible injury.

Dr. Boarman said that he had mentioned such a possibility in his paper.

Dr. J. B. Hamilton said it seemed the literature had been hastily treated. The autopsies made by Valsalva had not shown any intimate relation between concussion of the brain and hepatic abscess. Chelins says liver abscess and jaundice are common as late symptoms. Ballugall, Guthrie and Gross also mention it. Dr. Hamilton thinks the only explanation of its production has been that it is due to an irritation of the floor of the fourth ventricle and by the pneumogastric nucleus. He agrees with Dr. Boarman that it is a rare but long-recognized condition. Duret's examination showed extravasation in the fourth ventricle in a great many cases of concussion.

Dr. King could not see the connection between a jaundice resulting from old liver disease, as abscess, and the acute symptoms described in the paper.

Dr. Hamilton spoke in general terms, and in reply to Dr. Boarman said he thought that nervous jaundice, like diabetes mellitus, might be produced by compression in the fourth ventricle due to extravasation.

DOMESTIC CORRESPONDENCE

LETTER FROM NEW YORK.

(FROM OUR OWN CORRESPONDENT.)

The Bellevue Commencement—The Funeral of Dr. Flint—The Death of Dr. S. O. Vanderpoel—The University Commencement.

The commencement of the Bellevue School, which came off on the evening of March 15, was the saddest that this, or probably any other medical college, ever had. The week before had been buried the accomplished and popular young Demonstrator of Anatomy, Gaspar Griswold, and at the time of its occurrence the mortal remains of the immortal teacher and writer, Austin Flint, lay in their coffin awaiting the funeral services of the morrow. Under the circumstances the exercises were naturally of a memorial character, instead of the festal nature usual on such occasions. They were entirely private, being held in the lecture room of the Carnegie Laboratory, connected with the college, instead of some such place of amusement as the Metropolitan Opera House or the Academy of Music, and the building was elaborately and tastefully draped in the emblems of mourning. It was indeed a most affecting event. In his remarks to the graduating class, which numbered 140, the President, Dr. Isaac E. Taylor, spoke in feeling terms of the great bereavement of the college and the profession, and the eminent physiologist, Dr. John C. Dalton, now President of the College of Physicians and Surgeons, paid a most touching and eloquent tribute to the dead Professor.

The funeral services, which were held Tuesday, March 16, at Christ Church, of which Dr. Flint was a member, were of the greatest possible simplicity, and there were no pall-bearers. The church was completely filled with the Faculties and students of Bellevue and the other medical schools, representatives of the New York County Medical Association, the County Society and the Academy of Medicine, and citizens generally; and among those present were many distinguished men of other callings, as well as a number of well-known physicians from other cities. At the head of the coffin, beside which stood, during the whole service, the faithful negro servant of Dr. Flint, was a large and beautiful broken column, rising from a floral base, which was the offering of the

County Medical Association; and afterwards the flowers composing it were distributed amongst the patients in the medical wards of Bellevue Hospital, which the deceased had attended so long and faithfully and with signal benefit to the profession and suffering humanity.

It was before the County Association, which was a society especially cherished by Dr. Flint, that he read his last paper in public, that on "The Elements of Prognosis in Bright's Disease," which was published in THE JOURNAL of January 9 last. On Sunday, the day following his death, a special meeting of the Executive Committee of the Association was held at the house of the President, Dr. Leale, when the memorial which has already been published was adopted, and it was determined that the next meeting of the Society, which will occur April 19, should be exclusively devoted to commemoration exercises in his honor. It is expected that several of his friends and former colleagues in the different cities where he resided and won for himself not only distinction, but the sincere esteem and friendship of all his professional brethren, will be present on that occasion to render their tributes to his memory. At a meeting of the Commissioners of Charities and Corrections which was held on the following day, resolutions of sympathy were also adopted, in which special attention was called to the fact that Dr. Flint had been connected with the Department as visiting physician to the hospitals for twenty-five years. His loss is indeed an irreparable one.

On the day preceding Dr. Flint's death occurred that of another of the most distinguished physicians in the city and State, Dr. S. Oakley Vanderpoel. It will thus be seen that death has of late been very busy among the eminent men of the profession here; within a few short weeks no less than four having been called away from their labors, Drs. Post, Griswold, Vanderpoel and Flint. While Drs. Flint and Griswold were connected with the Bellevue school, Drs. Post and Vanderpoel belonged to the University; the former being Emeritus Professor of Surgery and President of the Medical Faculty, and the latter Professor of Public Hygiene.

The forty-fifth annual Commencement of the University Medical School was held at the Academy of Music on the 6th of March, when the degree of M.D. was conferred on 173 graduates. The class were fortunate in having to address them the Hon. Wayne MacVeagh, of Pennsylvania, who is everywhere known as one of the most attractive speakers in the country. In the course of his remarks he said: "If I were called upon to indicate as marvellous a change as any that has come over the knowledge of men in my time, I would say that it was the difference between the well educated and well equipped physician when I was born, and the man of the same relative rank and acquirements in his profession to-day. In almost every department of medical and surgical knowledge advancement has been made which has substantially revolutionized the art of healing and alleviating the diseases of men, and of prolonging human life." Later on he remarked: "And charity is the crowning glory of your profession. I have known a distinguished surgeon to leave the bedside of a sick President and make an exhausting journey in order to relieve the sufferings of an unfortunate person whose only possible means of recompense was gratitude." P. B. P.

ACADEMIC STUDIES IN RELATION TO MEDICINE.

TO THE EDITOR OF THE JOURNAL:

Dear Sir:—In this communication I propose to use the words "academic studies" in a general sense, as we may have occasion to use the words medical studies in a similar sense. By the former we understand those studies which are pursued in our common schools, high schools, military and naval schools, normal schools, academies, classical, scientific and literary schools, usually known as colleges. How many of these studies should be taken up by the student prior to the study of medicine, and how much time should be spent upon each of them, are questions which can only be answered by educated men of large experience. Those who have never faithfully pursued the study of mathematics, or languages, or the natural sciences, for a series of years, are not in a position to judge of their relative value as so many means of mental discipline or education. Hence, they have no right to express an opinion on the subject, any more than a non-medical man has a right to express an opinion in a case of pneumonia or syphilis. Other questions which imply a partial or a total exclusion of these or other branches of study from the curriculum of an academic course must be answered by the same kind of authority. Any new theory of preliminary education, advocated by a single profession or by a few men in a profession is scarcely worthy of serious consideration. The older methods may be somewhat modified to suit the age in which we live, but we are not moving so rapidly than we can afford to abandon the experience of our fathers in any department of activity, much less in the slow processes of educating the human mind.

If any one should ask where an expression of sentiment on the points mentioned or referred to may be found, we would say, that the statute books of all civilized nations (our own country being the exception) contain statements which cannot be misunderstood. The minimum requirements for matriculation in the theological, law, and medical schools of all European countries, the Canadian provinces, the South American States, and Mexico, consist in knowledge of the higher mathematics, at least three languages, belles-lettres, mental and moral philosophy and the natural sciences. Higher attainments are encouraged, and other studies are often pursued which are not required by law. The medical profession in the United States, whatever may be said to the contrary, cannot afford to occupy a lower plane than that of the profession in other great nations. In the direction of mechanical inventions we may have surpassed others, being stimulated by our potent laws, but this is no evidence that we can invent a better method of educating professional men.

The limited space at my disposal requires me to

be brief, and therefore I will confine myself to two or three additional paragraphs.

1. Our profession has thus far been under the necessity of making use of such academic institutions as the people of our numerous States have provided; and as far as it is possible for us to foresee, we will always be under the same necessity. We may, as a profession, control, in some measure at least, our medical schools, because there is no other class of citizens specially interested in them. Our military and naval schools, schools of design, normal schools and agricultural schools may also be regulated to suit the limited number of students who apply for admission; but no simple profession is likely to gain so great an ascendency in our country as to control the curricula of our numerous classical, scientific and literary institutions, about 300 in all; or to dictate what branches should be introduced and what excluded. The fact is, that in nearly all of them, the student has the choice of two or three courses of study, and there seems to be very little more needed to make them complete. The religious denominations which have very generally supervision over them, have very generously provided elective courses of study for those who do not desire to study theology; and many whose sons propose to study law insist, that they shall continue all through the junior and senior years, in the course in arts which they believe to be the best suited for mental discipline. A respectable number, though certainly not so large, enter upon the study of medicine with the degree of bachelor of arts. To suppose that the medical profession, in the United States which has ignored every standard, academic and medical, would support special schools with a fixed and limited curriculum is folly, and we do not hesitate to say it. Those who have energy to spare may waste it on such an issue, but we could not encourage them to do so. People generally place their sons in the care of those whose religious sentiments are in harmony with their own, if they cannot find a school in the neighborhood. I have now answered the question, *Where* should a student who desires a good education, prepare for the study of medicine? and I proceed to answer with equal brevity, another question not less important.

2. *What* shall a student, who desires a good education study until he is capable of deciding what he should do in life? This is not only an important question for every student to solve, but one which the faculties and trustees of our higher educational institutions are required to consider in all its bearings. For the parent to decide what shall be the life-work of his son before he has reached his majority would be as unwise as to give him full possession of his inheritence during his minority; and any system of education which would require the son to make so important a decision while yet in his minority, is false. We have all seen the minister in the pulpit, the attorney at the bar, and the medical doctor make a failure of life; and we have said, they have mistaken their calling. It is not sufficient to have merely an education—that is, a knowledge of angles and circles, Greek and Latin verbs, syllogisms and the laws of chemistry and biology. The student must have a natural fitness for his calling. A life-work must have some charm about it, to compensate for the annoyances incident to professional work, or disappointment and failure will result. It is plain that the student is not in a position to make so important a decision until he has reached his twentieth or twenty-first year, and even then he may make a fearful blunder, nor is the parent able to assist him. For the pulpit and the bar he should be an easy writer and a fluent speaker; he should have a clear voice and a pleasant address. For a successful study and practice of medicine, he should have a decided inclination in the direction of the natural sciences, especially biology combined with patience, self-possession and sympathy for mankind. A protracted course of academic study gives the student an opportunity to study himself. He will find, especially in the combined classical, scientific and literary course, opportunities to test his fitness for a place in the learned professions which he will not find in any other. In this course he is left entirely free, to study his mental capabilities and pecularities, and the indications of Providence. With these advantages, if he has chosen medicine, he will make more progress in one year than others will in two or three years; and he will not be constantly goaded with the thought that he is entirely ignorant of many branches of study which adorn the educated world. These and similar considerations have induced educated men in all civilized countries and for many centuries to present and advocate a combined classical, scientific, and literary course of study as the very best that can be devised for young men who desire to enter the learned professions.

In conclusion, allow me to say that it ill becomes the medical profession in our country to complain of our higher literary institutions, much less does it become us to speak of them in a disrespectful manner. In most of these schools instruction is gratis, or nearly so; besides it is entirely optional with the student whether he pursues one course of instruction or another. When our medical schools command as much respect as our academic institutions, they also will be liberally endowed. A high standard and an honorable policy will alone secure the confidence of those who have money to spare.

R. Lowry Sibbet, M.D.

Carlisle, Pa., March 7.

NECROLOGY.

AUSTIN FLINT, SR., M.D., LL.D.

Died at his residence in New York, on Saturday, March 13, Austin Flint, Sr., of cerebral apoplexy. He was stricken about midnight on Friday. Two or three weeks before he was called to Tarrytown to an important consultation, and on returning to the city found a pressure of business among his own patients. He was very much exhausted when the hour for a lecture at Bellevue arrived, but insisted on delivering it even while complaining of the burden he felt it. For several days after this strain his friends noticed that he seemed depressed and nervous, but he soon recovered his wanted energy, which was marvellous

for a man of his years. The last lecture for the term a few days ago was an hour long, and he followed it with a quizzing exercise which would have done credit to a professor of 40. He attended the examinations which were concluded on Friday, went to the Faculty meeting on Friday night, which lasted till a late hour, and was driven home apparently in the best of health and spirits. Very soon after reaching his home he was taken ill. Dr. E. G. Janeway was called in immediately, and seeing the nature of the attack sent for Dr. Austin Flint, Jr. Drs. Isaac E. Taylor and Wm. T. Lusk were called in consultation on Saturday, but the patient sank slowly, and died at 2 o'clock.

Dr. Flint was born in Petersham, Mass., on October 20, 1812, and was therefore in the seventy-fourth year of his age. He came of a family that had made a name in medicine in Massachusetts since the early settlement of New England. Thomas Flint, of Matlock, Derbyshire, England, the ancestor of Dr. Austin Flint, settled in Concord in 1638. Edward Flint, the great-grandfather of Dr. Flint, was a physician of reputation in the Massachusetts Colony in the early part of the last century; and his grandfather, Dr. Austin Flint, practised medicine in Leicester, Mass., until past the age of 90, dying in 1850. He laid aside his profession temporarily to enter the Revolutionary Army, but his services were soon needed as a surgeon. Dr. Flint's father, Joseph Henshaw Flint, was also a surgeon of reputation in Northampton and afterwards at Springfield. Dr. Flint pursued his academic studies at Amherst and Cambridge for three years, when he entered the medical department of Harvard College, and received the degree of M.D., in 1833. The first three years of his professional life were passed in Northampton and Boston, when he removed to Buffalo, N. Y., in 1836. He had already brought himself into prominence in the profession by his success as a practitioner and writer. In 1844 he was appointed to the chair of Institutes and Practice of Medicine in the Rush Medical College of Chicago, though still retaining his residence in Buffalo. This position he resigned at the end of a year. In 1846, he founded the *Buffalo Medical Journal*, and for ten years edited it with marked ability and signal success. His contributions to this periodical attracted attention on account of their unusual worth. Among the physicians practising in Buffalo at this time were Dr. James P. White and Frank H. Hamilton. With these, in 1847, Dr. Flint organized the Buffalo Medical College, which has also numbered among its professors, Drs. John C. Dalton and Sanford P. Hunt. In this institution Dr. Flint was Professor of the Principles and Practice of Medicine and of Clinical Medicine until 1852, in which year he was called to the Chair of Theory and Practice of Medicine in the University of Louisville. Here he remained until 1856, when he returned to Buffalo to take the Chair of Pathology and Clinical Medicine in the College which he had been instrumental in founding. In 1858 he went to New Orleans to teach Clinical Medicine, where he was also visiting physician to the Charity Hospital. He held these positions during the winters of 1858–1861, when he removed to New York to take the Chair of the Principles and Practice of Medicine and Clinical Medicine in Bellevue Hospital Medical College, and that of Pathology and Practical Medicine in the Long Island College Hospital to which he had been previously elected. He was also appointed one of the physicians to Bellevue Hospital. He relinquished the professorship in the Long Island College Hospital in 1868.

Dr. Flint went to New York distinguished as a practitioner, a teacher and an author. Besides his other published writings he had presented two essays to the American Medical Association on "The Variations of Pitch in Percussion and Respiratory Sounds" (1852), and "The Clinical Study of the Heart Sounds in Health and Disease," (1859), for which he received the first prizes of the Association.

When the National Medical Convention met in New York City on May 5, 1846, Dr. Flint registered as a delegate from the Buffalo Medical Association; the other delegate being Dr. Bryant Burwell. This National Medical Convention, it will be remembered, was the initiatory step towards the formation of the American Medical Association. Dr. Flint was one of the seventy-four who voted against a resolution, offered on behalf of the New York State Medical Society, setting forth that there was no mode of accomplishing the object of the Convention, and suggesting that it adjourn *sine die*. At this meeting he was appointed on a committee to report on a resolution, offered by the late Dr. Isaac Hays, for a uniform and elevated standard of requirements for the degree of M.D. in all the medical schools of the United States. The report of this committee, made in Philadelphia in 1847, at the second meeting of the Convention, is today a most interesting, applicable and valuable document.

Soon after this meeting of the Convention Dr. Flint, as already stated, helped to found the Buffalo Medical College; and seeing the difficulties under which medical schools labored in regard to subjects for practical anatomy, he introduced a resolution at the meeting in Philadelphia recommending that the medical profession of the different States unite in bringing their influence to bear on the legislatures to pass laws sanctioning and providing for dissection. At this meeting also he was placed on the Committee on Medical Literature for the next year, Dr. Oliver Wendell Holmes being Chairman of the Committee. By this time he was already known favorably as a writer. In 1849 he brought out an able article on the pathology of typhoid fever, giving an exact account of the intestinal lesions. In an article on diabetes, published in his *Buffalo Medical Journal*, in July, 1848, he had confirmed the opinion expressed by Todd, of England, in the *Provincial Medical Journal*, three months before, as to the pathology of this affection: "The presence of sugar in the blood and various secretions other than the urine render it inappropriate longer to rank diabetes among renal diseases." At the meeting of the *American Medical Association* in Cincinnati in 1850, for this name had been chosen at the Baltimore meeting of the National Convention, he was made Chairman of the Section of Practical Medicine; and the report on this subject shows how well he performed his work. By this time

several able papers had been given to the profession by him, notably two on serous effusion into the arachnoid cavity, which appeared in the *Buffalo Medical Journal*, in May, 1849, and April, 1850, and one on pleuro-pneumonitis complicated with pericarditis, in the same periodical in February, 1850, in which he again pointed out the occasional connection of acute cerebral symptoms with lesions confined to the heart or pericardium, and the errors in diagnosis which may be made during life. In November, 1849, he wrote a report on the epidemic of cholera in Buffalo in 1849. In the August, 1850, and succeeding numbers of his journal he gave a most elaborate analysis of fifty-two cases of typhoid fever, not only one of the most complete and valuable papers issued in the country during the year, but it remains to this day one of the most faithful expositions of the subject ever published in America. Of this paper the Standing Committee of the Association on Medical Literature reported at the meeting in Charleston, in 1851: "We cannot, in justice to the labor and talent displayed speak of it merely as affording a rigid comprehensive analysis of the subject. As an exposition of the clinical facts of the disease, as witnessed in this country, as an American work on fever, rich in material and admirable in execution, it is one of the best contributions ever published in the United States." His prize essay for 1852, on variations of pitch in percussion and respiratory sounds, has already been mentioned. On the title page of this essay was the following quotation from Andry's work on Diseases of the Heart: "Happy am I in my own estimation if I have thrown any light, in this Memoir, upon any clinical questions, and especially if I have stimulated the zeal of our young practitioners for the diagnostic studies which constitute, in my mind, one of the most beautiful parts of our art." In the same year he published his reports on Continued Fever; and in 1853 his clinical reports on Dysentery and on Chronic Pleurisy. These papers, including the prize essay, were translated into French and published in Paris in 1854.

Dr. Flint's success as an author was not the result of chance. An ever accumulating experience, added to patient work, with an analytical mind and a polished style, combined to make him one of the most agreeable of writers. His writings may be held up as models of scientific style. The conclusions drawn from facts were just and complete; the whole subject was stated fully, leaving nothing to be inferred. He seemed to feel it his duty to his profession to contribute his quota to medical literature, as it is the duty of every physician who draws freely from the common stock. At the meeting of the Association in Cleveland, Ohio, in 1883, Dr. Flint was elected President, though he neither sought nor desired the office, and his masterly address in Washington will always remain an object of careful study by those who have the best interests of the Association at heart. It was delivered just thirty-eight years after the first meeting of the National Medical Convention in New York, and opened with the words: "The American Medical Association has reached an age when the thoughts of one whose retrospections extend to its birth, naturally revert to the natal period of its existence. Of those who coöperated in the formation of the Association, not many now remain, and after a few more annual meetings all will have passed away." The objects of the Association, and the motives which led to its formation, are clearly and fully set forth. It will be remembered that Dr. Flint was made a member of the Committee on Medical Education at the first meeting of the National Convention; and in his address he ably discusses the practical question: "What can the Association do to promote more and more the elevation of the standard of medical education? He shows that this cannot be done by decrying the status of the profession. "As a body," he says, "the members of our profession in this country are neither ignorant nor in any respect unworthy. The profession is honorable and honored. In no other country is the social status of its members higher."

At the meeting of the International Medical Congress in London, in 1881, Dr Flint read a paper on "The Analytical Study of Auscultation and Percussion with Reference to the Distinctive Characteristics of the Pulmonary Signs;" a paper so suggestive and valuable that the brilliant and much lamented Mahomed suggested that a committee be appointed to report on a "Uniform Nomenclature of Auscultatory Sounds in the Diagnosis of Diseases of the Chest." Dr. Flint was made Chairman of the Committee, and the report was made at the meeting of the International Congress in Copenhagen, in 1884. But though he probably did more good work in the department of diseases of the chest than any man of the century —certainly as much—he was in no sense a specialist. He was not not only a born doctor, but an all-round one. His work in renal diseases was of the highest character, as will be seen from one article only, "The Elements of Prognosis in Bright's Disease," recently published in THE JOURNAL. His published works and papers are so familiar that it is unnecessary to give the list in this place.

At a meeting of the Faculty of the Bellevue Hospital Medical College held March 15, 1886, on motion it was resolved:

That this Faculty keenly sympathizes with the wife, son and family of our lamented *confrère*, Professor Austin Flint, in their irreparable loss of a tenderly devoted husband, father, and counsellor:

Yet that in their grief they will find consolation in that they can ever cherish the memory of his spotless life, his greatness and his goodness:

That the son, grandson, and great-grandson of eminent physicians and surgeons, entrusted by nature with medical skill and sagacity, so nobly fulfilled his mission:

That his powers of thought and action were preserved in their fulness of vigor to the close of his intellectual and benevolent career:

That his prayer was granted in being spared from lingering illness:

That as he had mitigated the sufferings of others he himself was saved from suffering:

That after a day and evening of arduous medical duties he retired to his painless couch of death—

"God's finger touched him and he slept."

Resolved, That this Faculty has been deprived in this dispensation of Providence of one of the most illustrious founders of this College, whose professors felt honored by having their names enrolled with his·

One through whose instrumentality other medical colleges have been established in our country;

One whose pupils fill chairs in our Faculty, while others hold distinguished positions in similar institutions of medical instruction;

One whose self-sacrificing and gratuitous services have been cheerfully rendered to the sufferers in Bellevue Hospital and other hospitals in our city and country for the last half century.

Resolved, that our city and the world has lost in his death one whose noble presence and tender sympathies in the sick-room cheered the heart and secured the confidence of the afflicted;

Whose remarkable record is justly the pride of any profession in any land;

Whose gifts and labors were recognized at home and abroad;

Who was honored with positions of distinction in America and Europe rarely won;

From whose eloquent lips thousands of students in many colleges have been taught the science of medicine, and by whose graphic pen tens of thousands have gained medical knowledge, and whose numerous and valued works translated into many tongues, will continue as fountains of instruction to future generations.

Resolved, That while our heads bow in grief at his sudden death, our hearts rise in gratitude to God for his prolonged life of widely extended usefulness.

Resolved, That a copy of these resolutions be engrossed and transmitted to his family, and that they be given for publication to the medical and secular journals of this city.

We may fitly close this sketch by quoting the remarks of Dr. Flint's long-time friend, Dr. John C. Dalton, at the Commencement exercises of the Bellevue Hospital Medical College: "I am sure there is but one thought in the minds of all who are here in this room this evening. A familiar and venerated presence no longer meets your eyes from its accustomed place. A voice to which you have listened, always with delight and profit, for so many years, is suddenly quiet on this returning anniversary. And yet I doubt whether our departed friend and counsellor ever wielded over his colleagues or class such an overwhelming influence as he does at this moment.

"He speaks to you to-night not with the imperfect utterance of an occasional discourse or a momentary topic, but with the complete and unmistakable language of a lifetime. He stands before you now in his entire character, ennobled by the record of his qualities and deeds, as the acknowledged representative of all that is best in the study, the teaching, and the practice of medical science and art. It was the universal verdict, from which I have never heard a dissenting voice, that among all the eminent men of the profession in this wide country, his was the one name which would be inevitably selected as the first. His single-minded devotion, untiring industry, and indomitable strength of purpose raised him long ago to the position which he held to the last day of his life. And now, after conducting your studies through the session just closed, he has graduated before you, he has taken his final and highest degree, conferred by the Power that is Supreme over us all; and the parchment of his biography now bears the stamp of Emeritus.

"For you, gentlemen of the medical class, I am sure that Dr. Flint's teachings are far from being ended. I can wish you nothing better than that you carry them with you throughout the future, and that you never cease to remember his instructions and to emulate his life."

MISCELLANEOUS.

MEDICAL SOCIETY OF THE STATE OF TENNESSEE.—The fifty-third annual meeting of the Medical Society of the State of Tennessee will be held in Memphis, commencing Tuesday, April 6, 1886.

Dr. D. D. Saunders, of Memphis, is Chairman of the Committee of Arrangements.

Members who cannot attend the meeting can retain their membership and receive a copy of the Transactions by forwarding $1 to the Treasurer.

Members or delegates who desire to reach Memphis over the Louisville and Nashville system, should purchase regular tickets to Memphis and procure from the ticket agent, when these tickets are purchased, a certificate to this effect, which should be filled up by the Secretary of the meeting to show that the person named was in attendance and entitled to special rate, and upon presentation of the same to the agent at Memphis, he will sell return tickets at one-third fare. The E. T. Va. & Ga. and the M. & C. will sell tickets to Memphis, at three cents per mile, issuing to each purchaser a certificate, on which, if properly signed by the Secretary of the Society, and presented to the ticket agent at Memphis, he will sell a return ticket at one cent per mile. Tickets to be sold 3d, 4th, 5th and 6th of April, and return tickets to be sold 7th, 8th, 9th and 10th.

C. C. FITE, *Secretary*.

THE AMERICAN SURGICAL ASSOCIATION will meet in Washington, D. C., on April 28, 29, 30, and May 1.

THE FRENCH SURGICAL CONGRESS will be held in Paris on October 18 to 24 inclusive.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM MARCH 13, 1886, TO MARCH 19, 1886.

Capt. Jno. Van R. Hoff, Asst. Surgeon, ordered from Dept. Cal. to Dept. Mo. (S. O. 60, A. G. O., March 13, 1886.)

Robertson, R. L., 1st Lieut. and Asst. Surgeon, granted leave of absence for one month. Fort Ringgold, Tex. (S. O. 29, Dept. Tex., March 8, 1886.)